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THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

THE CONTRIBUTIONS OF AMERICA TO GEOLOGY[1]

IN speaking of the contributions of America to geology, I do not propose to give an inventory of the geological facts which have been made known as the result of work in this country. An area of three million square miles has been covered by geological reconnoissance, and much of that area has been surveyed in detail. It is, moreover, an area interesting in many respects. It is a country of vast mineral wealth, leading the world in the production of coal, petroleum, salt, iron, copper, silver and lead, and ranking with Australia and South Africa as one of the three great gold-producing regions of the world. It is a country presenting a remarkable variety of topography and geological structure; and some of its scenic features can be adequately described only in superlatives. It includes vast prairies of great fertility; the Missouri-Mississippi river system, with thousands of miles of navigable waters; the broad and complex mountain mass of the Cordillera; the Great Lakes of the Canadian border, a chain of fresh-water seas; the sublime cataract of Niagara; the remarkable region of interior drainage of the Great Basin; the geysers of the Yellowstone; the cañon system of the Colorado and its tributaries, unparal-

[1] Address of the vice-president and chairman of Section E—Geology and Geography, American Association for the Advancement of Science, New York meeting, December, 1906.

leled in depth and extent; the lava flood of the northwest, surpassed in area only by that of the Deccan. The exploration of a region so vast and so varied must, of course, have made an important addition to the facts upon which geological science is founded.

Of these facts, however, I do not propose to give even an outline. I propose rather to ask the question, 'What has our country contributed to the stock of geological ideas?' In that classical history of geological science which Lyell has given us in his 'Principles of Geology,' he calls attention to the fact that the share which different nations bore in the early development of geological science was dependent not alone upon the genius of individual workers, but in large measure upon the peculiar geological conditions of the various countries in which they worked. It was in the presence of the varied mineral wealth of the Erzgebirge that Werner laid the foundations of mineralogy and lithology; the magnificent display alike of igneous and of aqueous agencies in the Highlands of Scotland helped to guide Hutton to those theoretical views which were the beginning of modern dynamical geology; the remarkable completeness with which, for an area so small, the series of stratified formations is developed in England, gave William Smith the opportunity to lay the foundations of stratigraphical geology; and the abundant vertebrate fossils in the Paris basin enabled Cuvier to create the science of paleontology. That history of the labors which founded our science in the close of the eighteenth and the beginning of the nineteenth century, suggests the question whether there have been developed any characteristically American ideas in geology.

Of course, it must be admitted that there is to-day no department of geological science which is as characteristically American as mineralogy was German, and dynamical geology was Scotch, and stratigraphical geology was English, and paleontology was French, a century ago. The conditions of life in colonial days and in the early decades of our national history did not afford the opportunity for fruitful scientific investigation. We had no universities, no libraries, no museums, that could be compared with those of the Old World. The conquest of the land from forest and wild beast and savage man, the achievement of national independence, the beginning of the development of national wealth, necessarily preceded the beginning of scientific investigation. When the time came for scientific work in this country, geology had already become a recognized science. That early stage of incompleteness in which one department was developed here and another there, had passed. It was too late for any country to create a new department in geology. I believe, nevertheless, that there have been certain contributions to the stock of geological ideas which are characteristically American; and that it is not fanciful, in the spirit of that passage of Lyell which I have cited, to connect these characteristically American ideas with our geological environment.

I. THE PERMANENCE OF CONTINENTS

Among the early state geological surveys there were two commenced in 1836 which were destined to have in different ways a surpassing influence upon the development of geological thought. Those were the surveys of New York and Pennsylvania. In passing southwestward across New York, from the Adirondacks to the Pennsylvania border, one would traverse substantially the whole series of Paleozoic formations from the Cambrian to the Carboniferous, most of these formations extending across the state in approximately parallel east

and west bands. That remarkably complete development of the Paleozoic series made the stratigraphy of New York a standard of comparison for the subsequent study of the Paleozoic strata throughout the eastern United States, as the stratigraphic series of England had already become, in its broader outlines, a standard of comparison for the world. But that regular succession of Paleozoic strata from the Adirondacks to the Pennsylvania border was suggestive of a dynamic idea. It suggested a gradual emergence of the land from the waters of the sea. In later years, as geological investigation extended westward, a somewhat similar succession could be traced in the Mississippi basin, southward from the Archæan mass north of the Great Lakes. The doctrine of the permanence of continent and ocean—the gradual emergence of continental lands and the withdrawal of the waters into the deepening ocean basins—was first enunciated by Dana in 1846. He had just returned from his voyage around the world in Wilkes's exploring expedition. In that voyage he had studied the phenomena of barrier reefs and atolls, had adopted Darwin's theory of their origin by subsidence, and had defended and illustrated the theory by a far greater wealth of observation than Darwin's route had afforded him the opportunity to make. It was, apparently, the thought of the subsiding ocean bottom, rather than the thought of the emerging land, by which Dana was first led to the doctrine of the permanence of continent and ocean; but, in his presidential address before the American Association for the Advancement of Science, in 1855, Dana refers to the stratigraphy of New York as illustrating the idea of continental emergence. The generation of students who have learned geology from Dana's 'Manual' and 'Text-book,' will remember how prominently he brings forward the succession of Paleozoic formations in New York, as illustrating his conception of the gradual emergence of the land from a continental sea.

The doctrine of the permanence of continents when announced by Dana was essentially a new one. Geologists and pseudo-geologists of all classes had felt at liberty to redistribute continents and oceans according to their own sweet will. After the biblical pseudo-geologists had become convinced of the impossibility of the deposition of the whole series of fossiliferous strata in the Noachian deluge, their next shift was the supposition that the fossiliferous strata had been deposited in the ocean in the interval between the creation and the deluge, and that at the time of the latter event continent and ocean were reversed. Hutton believed that the débris of the continents was carried far out to sea by means of ocean currents, and was deposited over substantially the whole floor of the ocean; and, when one continent was worn away, another might be uplifted in some other part of the world. Lyell eliminated the catastrophic element of Hutton's theorizing; but, like his predecessor, Lyell believed in an indefinite amount of change in the distribution of continent and ocean. In attempting to find a geological explanation for changes of climate, he felt at liberty to speculate on a series of changes in the distribution of continent and ocean which would sometimes bunch the continents around the poles, and at other times girdle the earth with an equatorial belt of land. The readers of Darwin's 'Letters' will remember his half comic, half pathetic protest, in a letter to Lyell, that the disciples of the great geologist 'in a slow and creeping manner beat all the old catastrophists who ever lived.'

There is now little doubt that Dana was right in his general conception. The greater density of the suboceanic masses in com-

parison with the subcontinental masses, as shown by pendulum observations, indicates that the distinction between continent and ocean has its basis in the heterogeneity of the material in the interior of the earth; and the determining conditions must, therefore, have had their origin in the initial aggregation of that part of the primitive nebula which formed the earth; or, perhaps, as suggested by Chamberlin and Salisbury, in changes attendant upon the beginning of the formation of the ocean. The study of the sedimentary rocks which cover our existing continents shows that almost all of them were deposited in shallow waters; many of the strata, indeed, in waters so shallow that the layers of mud and sand were from time to time exposed by the receding tide or the subsiding freshet, to dry and crack in the sun or to be pitted by raindrops. None of the sedimentary deposits seem to have been formed in waters of truly oceanic depth.

Certain it is, however, that Dana made the evolution of the continents too simple an affair. He recognized, indeed, that the progressive emergence of the continental lands was attended by continual oscillation; yet, even in the last edition of his 'Manual,' it appears that he did not duly appreciate the magnitude of those oscillations. We now know that in early Cambrian time the Mississippian sea was only a sound or strait, most of the area in which the Trenton limestone was subsequently deposited being then dry land. Only gradually did the Appalachian strait widen out into the Mississippian sea. A true conception of continental evolution must recognize two complementary truths: a wide range of oscillatory movement, and yet on the whole a progressive deepening of ocean basins and a progressive emergence of continental lands. Chamberlin has formulated the doctrine of an alternation of marine and continental periods due to an intermittently progressive deepening of the ocean basins, and has connected therewith an ingenious and beautiful theory of climatic changes in geological time.

The doctrine of the progressive evolution of continents, as taught by Dana, gave new clearness and emphasis to the general conception of geology as a history of the globe. Le Conte, in his cordial and generous eulogy of Dana, declared that 'geology became one of the great departments of abstract science, with its own characteristic idea and its own distinctive method, under Dana.' There is certainly somewhat of exaggeration in this commendation, yet the statement contains an important truth. More or less clearly, all geological investigators must have felt that the distinctive idea of geology is that the structures of the rocks of the earth's crust have their supreme significance as monumental inscriptions, the deciphering of which may reveal to us the history of the earth. Yet this conception was never before so clearly formulated, and the whole treatment of the subject so consistently adjusted thereto, as in the writings of Dana. The portion of previous manuals dealing with the local distribution of the series of strata had generally borne some such title as 'Stratigraphical Geology'; and very commonly, as in the well-known works of Lyell and De la Beche, the series had been traced backward, beginning with the most recent strata. In Edward Hitchcock's 'Elementary Geology,' with which, in my boyhood, I commenced the study of the science, the stratigraphic chapter bears the title, 'Lithological Characters of the Stratified Rocks.' It occupies only twenty pages in a book of more than four hundred pages. It traces the formations backward in Lyellian fashion. Separate from the stratigraphic chapter, is another and longer chapter on paleontology, which is arranged botanically and zoologically, and not chro-

nologically. The phrase, 'Historical Geology,' which forms the title of the largest section of Dana's 'Manual,' involves a distinct clarification of the general view of the science. Starting with this conception, he, of course, dealt with the earliest formations first. In the treatment of each era he endeavored to reconstruct, from the evidence afforded by the kinds and distribution of the rocks, the physical geography of the time. The subdivisions of that chapter of the 'Manual' are characterized, not as series, systems and groups of strata, but as eras, periods and epochs of time. The common use, in recent geological writings, of such phrases as 'Silurian era,' rather than 'Silurian system,' etc., is a testimony to the influence of Dana's mode of treatment.

II. THE THEORY OF MOUNTAIN-MAKING

The Geological Survey of Pennsylvania, to which I have already referred, made known the folded structure—the alternate anticlines and synclines—of the Appalachians. The beautiful sections of these folded strata, in the Atlas of that survey, reveal the thoroughness with which the structure of the mountains was investigated by Henry D. Rogers; and can be studied with delight to-day, in spite of the fantastic nomenclature, which the student has to translate into the familiar language of the New York Survey. In those sections it appears that, in general, each fold is unsymmetrical, the dip on the northwest side being steeper than that on the southeast, if, indeed, the dip on the northwest side is not carried beyond the perpendicular and reversed. While in each fold, as a rule, the dip is steeper on the northwest side than on the southeast, if we compare the successive folds, we find the dips growing more gentle as we go from southeast to northwest. The nearly vertical or overturned dips of the folds on the eastern border pass by a gradual transition into gentle undulations on the western border of the Appalachian zone.

While the stratigraphy was worked out so beautifully in the first Geological Survey of Pennsylvania, the dynamic conception derived from it was crude indeed. The conclusions of the author are summed up in the following sentences: "The wave-like structure of undulated belts of the earth's crust is attributed to an actual pulsation in the fluid matter beneath the crust, propagated in the manner of great waves of translation from enormous ruptures occasioned by the tension of elastic matter. The forms of the waves, the close plication of the strata, and the permanent bracing of the flexures, are ascribed to the combination of an undulating and a tangential movement, accompanied by an injection of igneous veins and dykes into the rents occasioned by the bendings. This oscillation of the crust, producing an actual floating forward of the rocky part, has been, it is conceived, of the nature of that pulsation which attends all great earthquakes at the present day." The wave-like form of the Appalachian anticlines and synclines is a beautiful generalization of accurate and conscientious observation; but the dynamic theory suggested for its explanation needs to-day no other refutation than its simple statement.

But, however completely the Pennsylvania geologists failed to construct a satisfactory theory of mountain-making, their observations of Appalachian structure were of immense value in their destructive effect upon some of the notions of mountain-making prevalent at the time. In the text-books in the early part of the nineteenth century, a diagram often appeared representing a transverse section of an ideal mountain range. A vertical wall of crystalline rock forms the center and the crest of the range. Against this wall, on each

side, the oldest strata lean in a nearly vertical position; farther from the center, strata of less antiquity lean with more gentle dips against the steeper and older strata; and finally, on the flanks of the mountain range, still newer strata rest nearly horizontally. The diagram represents a dislocation of the strata by repeated vertical upthrusts of the crystalline core. Of course, it was seen at once that there was nothing common to these ideal sections and the real sections of the Appalachians revealed by the Pennsylvania Survey. The billowy form of the Appalachian folds meant something, and something very different from the prevalent conception of mountain ranges.

The true interpretation of the Appalachian waves is probably to be found in the contractional theory of mountain elevation, of which Dana was the leading expounder. The views of Le Conte on the subject of mountain-making were in most respects similar to those of Dana. But, while Le Conte's discussions were of great value, the priority in the general development of the theory belongs to Dana. "To the North American geologists," says von Zittel, "undoubtedly belongs the credit of founding the theory of horizontally acting forces and rock-folding upon an ample basis of observation."

That the main cause of mountain elevation is tangential pressure in the crust resulting from internal contraction, is now generally acknowledged, though there may be doubt whether the main cause of contraction is the cooling of the earth from an incandescent condition, as assumed in the commonly accepted form of the nebular theory, or the gravitational adjustment of an incoherent mass of meteors, as assumed in the more recent planetesimal hypothesis of Chamberlin and Moulton.

The idea of the contractional origin of mountains was not, indeed, original with Dana. There was a glimmer of the idea in the writings of Leibnitz, and Constant Prévost developed the idea into a definite scientific theory; but the elaboration of the theory into its present form we owe chiefly to Dana. His discussion of the subject began in the *Journal of Science*, in 1847. In later years he returned to the subject again and again; and the theory, as shaped by his maturest thought, appears in the last edition of the 'Manual.' In his earlier writings his views of the origin of continents and mountains were developed on the assumption of a liquid globe. In later years he abandoned that view, and adjusted his theories to the more probable doctrine of a globe substantially solid.

The conception of the subsidence of the suboceanic crust, which led Dana to his views of the permanence of continent and ocean, is an important element in his theory of mountain-making. In the contraction of the earth's interior, the suboceanic crust necessarily flattens in its subsidence, so that its section continually approaches the chord of the arc, thus exerting a tangential thrust toward the continental areas. The rather abrupt change in the radius of curvature in passing from the oceanic to the continental areas, makes the continental borders lines of weakness which determine in general the location of the great mountain wrinkles. In the two continents of North and South America, with their mountain borders on the east and west and their vast interior plains, Dana found exemplified the typical continent. Their isolated situation seemed to allow a more typical development than was possible in the Old World, where continents are massed together; as the laws of crystalline form can exhibit themselves in perfection only where a single crystal in a solution or magma is allowed to grow without interference of other growing crystals.

According to the views of Dana and

Le Conte, mountain ranges have been only exceptionally developed by geanticlinal uplifts of the earth's crust. A mountain range, in general, has its birth in a geosyncline—a downward folding of the crust, forming a trough, in which sedimentation goes on *pari passu* with the subsidence. At length, after long ages of subsidence and sedimentation, the strata in the trough are crushed together into alternate anticlines and synclines, or one part is forced over another in great thrust faults, while slaty cleavage and more decided metamorphism may be produced. The mountain range thus produced Dana called a 'synclinorium,' while he suggested the name 'anticlinorium' for a mountain range formed by a permanent geanticlinal elevation. Apparently the actual history of most mountain ranges is complex. The Appalachian range, for instance, was formed as a synclinorium at the close of the Paleozoic, degraded nearly to base-level in Mesozoic time, and again elevated by a broad geanticlinal movement early in Cenozoic time. It represents, therefore, in its complex history, Dana's two types of the synclinorium and the anticlinorium.

There are unquestionably weak points in the theory of mountain-making as developed by Dana and Le Conte; and, in our ignorance of the conditions in the interior of the earth, and of the forces there in action, it ill becomes us to be dogmatic; but the contractional theory seems worthy of provisional acceptance as the most plausible explanation of orogenic movements yet suggested.

III. GLACIAL GEOLOGY

Surely no part of the world affords better opportunity for the study of glacial geology than America. Its ice sheet, four million square miles in area, far exceeds any of the ice sheets of the Old World. The imbricated sheets of till in the Mississippi Valley afford clear evidence of the complex series of glacial and interglacial epochs. The driftless islands in the vast area of till, and the interlobate moraines, show the division of the marginal portion of the ice sheet into lobes determined by the topography. The old beaches along the shores of the Great Lakes, the living Niagara, and the various extinct Niagaras, record the stages of the melting of the ice. The Malaspina Glacier affords illustrations of the formation of eskers and of other phenomena which must have marked the stagnant margin of the waning ice sheet. Surely our country affords most favorable conditions for the study of the history of the Glacial period.

It is a curious fact that the first published suggestion of the agency of ice in connection with the drift came from a cotton manufacturer in Connecticut, Peter Dobson by name. In the *American Journal of Science,* in 1826, he gives a very clear and satisfactory description of the glaciated boulders observed in the drift, and gives as his conclusion: "I think we can not account for their appearances unless we call in the aid of ice along with water, and that they [the boulders] have been worn by being suspended and carried in ice, over rocks and earth, under water." It was his idea that these boulders were lifted from the bottom of the sea by sheets of anchor ice. This theory was certainly more satisfactory than most of the theories in vogue before that time, and more satisfactory than many of the opinions held at a later date.

The credit of the introduction and championship of the glacier theory of the drift belongs, not to a native, but to an adopted citizen of this country. Louis Agassiz came to this country in 1846. In his early home in Switzerland, he had already adopted the belief of Venetz and Charpentier in the former great extension of the Alpine

glaciers, and their agency in the transportation of blocks from the Alps across the Swiss lowland to the Jura. It is, indeed, a curious fact that these Swiss geologists were anticipated, in the conception of the transportation of these blocks through the agency of glaciers, by Playfair, who suggested the idea in 1802. In the early papers of Agassiz the conception of the Glacial period took a form which he himself later recognized as an exaggeration. He conceived at first of a fall of temperature so wide-spread and so extreme that a polar ice cap extended southward over the whole breadth of Europe and across the Mediterranean, reaching the Atlas Mountains. Later he recognized the ice sheet that covered the Alps as entirely separate from the ice sheet of northern Europe. The tendency to an exaggerated view of the Glacial period overcame him again in later years, when he maintained that, at the climax of the Glacial period, there was 'floating ice under the equator, such as now exists on the coasts of Greenland.' Incidentally, he based upon this extravagant conception of the Glacial period an argument against Darwin's views of the origin of species, maintaining that the wide-spread cold of the Glacial period produced a general extermination of life, necessitating a new creation. But the extravagance of some of his conceptions, and his vain attempt to stop the resistless progress of the doctrine of evolution, may well be forgiven, in memory of the great service which he rendered in bringing into general acceptance the glacier theory of the drift.

As Agassiz traveled in various parts of his adopted country, he recognized everywhere in the northern states the traces of glaciation, already familiar to him in Switzerland and in Scotland; and his views found more ready acceptance in this country than in some of the countries of Europe. Guyot, who had been associated with Agassiz in the study of the glaciers of Switzerland, came to this country in 1848; and he was, of course, a strong ally in the defense of the glacier theory. As early as 1841 Edward Hitchcock had been so strongly impressed by the writings of Agassiz that he recognized clearly the traces of glaciers in Massachusetts; and, in his presidential address before the Association of American Geologists, and in the postscript to his 'Final Report on the Geology of Massachusetts,' he shows himself almost persuaded to adopt the glacier theory for the explanation of the drift in general. He could not, however, quite bring himself to the acceptance of the conception of a glacier capable of moving with so little slope as to be able to transport material southward over the whole of northeastern America; and he accordingly limited the action of glaciers to those cases in which the drift appeared to be dispersed somewhat radially from local centers in mountainous or hilly regions. The general southward movement of the drift seemed to him to require the conception of submergence of the land, and transportation by icebergs floating southward from the Arctic regions. As early as 1852, one of the best and most popular text-books of the time, that of Gray and Adams, gave the preference to the glacier theory of the drift. Dana, in his presidential address before the American Association for the Advancement of Science, in 1855, manifestly inclined to the glacier theory; and, in the first edition of his 'Manual of Geology,' in 1862, he gave his adhesion more decidedly to that view. The next generation of American students of geology were brought up on the various editions of Dana's 'Manual' and 'Text-book,' so that thenceforward the glacier theory was recognized in this country as orthodox. In some of the countries of Europe the theory of submergence and transportation

of boulders by icebergs held the ground longer. In 1864 Lyell still maintained the submergence of the plain of northern Europe, and of the glaciated region of North America; and it was not until 1875 that Torell, in a memorable meeting of the German Geological Society, convinced the German geologists that the drift of northern Germany was transported by an ice sheet whose center was in the mountains of Scandinavia, and the name Diluvium, though still used in German geological writings, was completely emptied of its original connotation.

Within the last few decades the labors of a large number of earnest and able investigators have developed the glacier theory more in detail, and have added vastly to our knowledge of Quaternary history. The imaginary polar ice cap has given place to ice sheets of more limited dimensions, though still vast, developed respectively about the Laurentide, Keewatin and Cordilleran centers. The series of terminal moraines, marking stages of readvance or halts in the retreat of the ice sheet, have been carefully mapped. From the experience gained in the study of terminal moraines in this country, Lewis was enabled, in 1886, to recognize and interpret the terminal moraines of the ice sheet in England; and Salisbury, in the following year, those of north Germany. The recognition of interlobate moraines and of driftless areas led to a clearer understanding of the nature of the movement of the great ice sheet. The driftless area of Wisconsin was noticed by J. D. Whitney as early as 1862, and has been more carefully studied by Chamberlin and Salisbury; and the latter investigator has called attention to a smaller driftless area in Illinois. The Quaternary period, instead of being brief and comparatively simple, has been shown to be of long duration and great complexity. It has been analyzed into a succession of glacial and interglacial epochs; and, from the vast amount of erosion in some of the interglacial epochs, it has been inferred that post-Glacial time is very short in comparison with inter-Glacial time. The history of the series of lakes held between the front of the receding ice sheet and the southern water shed of the Saint Lawrence basin, has been studied by Gilbert, Taylor, Fairchild and others. In Chamberlin's theory that the cause of the Glacial climate is primarily the diminution of the amount of carbon dioxide in the atmosphere, and that the location of the main centers of glaciation is due to the path of cyclonic storms, we have a theory which, if it can not be accepted with full confidence as the true one, is at least the only theory of the Glacial climate which has not yet been weighed in the balance and found wanting. There seems still to be some disagreement among physicists on the critical question, what effect small changes in the amount of carbon dioxide in the atmosphere would produce upon the climate. The views of Arrhenius, upon which the theory of Chamberlin is based, have been contested by Angström and Very, and are not accepted by Hann. If the physicists can be brought to an agreement on this fundamental point, we may feel that we have, at last, a theory of the Glacial climate. The alternative at present seems to be the acceptance of Chamberlin's theory, or the confession that we have as yet no explanation of the Glacial climate.

IV. SUBAERIAL DENUDATION AND THE EVOLUTION OF DRAINAGE SYSTEMS

In early years the study of geology in this country was substantially confined to the region east of the Mississippi; but, in due season, the weird and fascinating region of the Cordillera revealed itself to explorers and geologists. It is now more than half a century since American geolo-

gists began the study of that western wonderland. The earliest geological work was done in connection with expeditions undertaken for other purposes, as the Pacific Railroad explorations, which commenced in 1853, and Ives's Colorado expedition. It is, however, forty years since the national government established geological surveys in that western country. A period of a dozen years commencing with 1867 was marked by the achievements of four great organizations devoted specifically to geological work—the Survey of the Fortieth Parallel, the Survey West of the One Hundredth Meridian, the Survey of the Territories and the Survey of the Rocky Mountain Region. Since 1879 all these organizations have been superseded by the United States Geological Survey. A new world for geologists was that weird western land—that land of deserts, plateaus and cañons, with vast stretches of almost horizontal stratification broken by faults and monoclines, revealing its geological structure with wondrous clearness by reason of an arid climate whereby it has been left naked and destitute of any mantle of soil and vegetation. There is revealed, as nowhere else in the world, the power and the method of subaerial denudation. The Grand Cañon of the Colorado is a stupendous object lesson of erosion; and, if one is compelled to recognize river erosion in the cañon itself, scarcely less strenuous is the compulsion to recognize subaerial denudation in the vast platform of Carboniferous strata with little outlying buttes of Permian and later formations. The study of that country has been fruitful in its contributions to the knowledge of aqueous agencies in dynamical geology. Very early appeared those three monumental works: Powell's 'Exploration of the Colorado River,' Gilbert's 'Geology of the Henry Mountains' and Dutton's 'Tertiary History of the Grand Cañon District.' The value of those early studies in a new field is not greatly lessened if some conclusions must be modified by later study. It may be that the course of the Green River through the Uinta Mountains is not a typical case of antecedent drainage; and it seems certain that the esplanade in the Kanab section of the Grand Cañon is not due to a long pause in the movement of elevation, in which the river nearly attained base-level: but the conceptions introduced into dynamical geology as the result of those early studies are no less valuable and fruitful.

The first lesson that geologists learned in that western land was the efficiency of subaerial denudation—the power of atmosphere, rain and river to remove vast quantities of material, and shape the topography of wide areas. From Hitchcock's 'Elementary Geology' I learned in my boyhood, in regard to cracks and fissures in the strata, "If the fissure is open and of considerable width, it is called a gorge; if it be still wider, with the sides sloping or rounded at the bottom, a valley is produced." Hitchcock was convinced, indeed, that the Niagara gorge was due to erosion; but he declared that the gorges of the Connecticut between Mount Tom and Mount Holyoke, and below Middletown, could not be due to the action of the river, and that the gorges of the same river at Bellows Falls and Brattleboro were too wide to have been formed by the river alone. In his 'Illustrations of Surface Geology,' in discussing the criteria by which to distinguish fluviatile from oceanic work, he declared, "Rivers have little power to form wide valleys." Apparently geologists were confused with a vague idea that the ocean is bigger than the rivers, and that, therefore, it can do more work in erosion. As early as 1847, Ramsay had recognized that the summits of the mountains of Wales were remnants of a former plain of erosion,

but he conceived of it as a 'plain of marine denudation,' and that phrase holds its place to-day in English writings. With genuine British loyalty and conservatism, Sir Archibald Geikie retains the phrase even in the latest edition of his 'Text-book,' in explanation of table-lands of erosion; though, even in his earliest edition, he declared that, in the production of plains of marine denudation, "the sea has really had less to do than the meteoric agents. A plain of marine denudation is that sea-level to which a mass of land has been reduced mainly by the subaerial forces." He attributes to ocean waves and currents only 'the last touches in the long process of sculpturing.' Elsewhere Geikie bears emphatic testimony to the influence of American geologists, in the words: "Unquestionably the most effective support to Hutton's teaching has been given by the geologists of the United States, who, among the comparatively undisturbed strata of the western territories, have demonstrated, by proofs which the most sceptical must receive, the potency of denudation in the production of the topography of the land." It is, indeed, marvelous with what prophetic vision Hutton and Playfair conceived some of those ideas of river action which the geologic world in general learned only from the work of American geologists in the Cordilleran region. On this subject, those two Scotchmen were a half-century in advance of their time.

That western land has taught us not only to recognize the fact of subaerial denudation, but also to formulate its methods. In Powell's 'Exploration of the Colorado River,' he distinguished rivers as consequent, antecedent, and superimposed. Davis has carried the analysis somewhat further, giving us subsequent and obsequent rivers. Powell formulated the doctrine of base-levels; Davis has given the conception greater accuracy and consistency by distinguishing base-level from profile of equilibrium. The base-level of a district is a portion of the ideal spheroidal surface of the earth. Disregarding the curvature of the earth's surface, base-level may be represented in profile as a straight line; profile of equilibrium as a curve, concave upward, tangent to base-level at the mouth of the river, gradually approaching base-level with the progressive denudation of the country, but never quite reaching it except at the point of tangency. To Davis also we owe the full development of the conceptions of youth and age in river valleys and in drainage systems, and of cycles of erosion ending in the formation of peneplains. We have learned to search in every rugged mountain region for remnants of ancient peneplains. We have learned, in general, that geological history is to be read, not only in deposits, but also in erosion forms.

V. TERTIARY MAMMALS AND THE DOCTRINE OF EVOLUTION

Half a century ago the exploring expeditions connected with the Smithsonian Institution began to collect fossils from the Tertiary deposits of the western plains. Later the work was followed up by the Geological Surveys under the auspices of the national government, and by numerous private expeditions under the auspices of universities, scientific associations and individuals. Over those western plains were found to stretch vast continental deposits, certainly not all of lacustrine origin, as at first reported, but in part piedmont alluvial formations, in part eolian deposits, and, in limited areas, deposits of volcanic dust. These continental deposits of the western plains yielded in unparalleled richness mammalian fossils, which have been studied by Leidy, Marsh, Cope, Osborn, Scott, Wortman, and others. No other single series of discoveries has been so

potent in changing the bearings of paleontology upon the doctrine of evolution.

In Darwin's two chapters on geology in the 'Origin of Species,' he marshaled with great skill the geological facts then known which appeared favorable to evolution. Yet he recognized in the facts of paleontology 'perhaps the most obvious and serious objection which can be urged against my theory.' He cited a long list of recognized authorities in geology and paleontology, still living or recently dead at the time of the publication of his first edition, who were believers in the immutability of species—Cuvier, Agassiz, Barrande, Pictet, Falconer, Forbes, Lyell, Murchison, Sedgwick. Of these Lyell alone lived to become a convert to evolution.

Of course the objection which Darwin felt so strongly himself, and which seemed conclusive to so many paleontologists at the time, was the absence of gradation between different forms. The theory of evolution, and especially the strictly Darwinian form of that theory, requires fine gradation between species—not indeed between different species now existing, but between existing species and species now extinct, and between fossil species of successive periods. In general, such gradations do not appear. Fossil species are about as sharply defined as recent ones; and whole groups of species—orders, classes, sub-kingdoms—have appeared without recognizable ancestry. Darwin's answer to this objection was given in the phrase now become classical, 'the imperfection of the geological record.'

In the half-century since the publication of Darwin's first edition, the attitude of paleontologists has completely changed. Not only is it true at present that paleontologists are substantially unanimous in accepting the doctrine of evolution; but it has come to be generally believed that the very science which afforded a half-century ago the strongest objection to evolution now affords its strongest support. This change is in large part due to the discoveries which have so shattered the objection that once appeared so strong. Innumerable links then missing have been brought to light. Intermediate forms between orders and classes formerly supposed to be widely separated from each other have been discovered in great abundance. Numerous series of genera may be traced through successive geological periods, exhibiting a gradually progressive change which almost irresistibly suggests to the mind the belief that the series are truly genetic. The fossils of our western plains have afforded a goodly share of the most important of these new evidences of evolution.

When the first edition of the 'Origin of Species' was published, the classes of birds and reptiles seemed to stand widely asunder. But in the very next year (1860) an odd feather of Archæopteryx was discovered, and a year later the skeleton now preserved in the British Museum. But Archæopteryx was a solitary representative of the birds of markedly reptilian character until the discovery of Ichthyornis and Hesperornis in the Cretaceous of Kansas, of which preliminary descriptions were published by Marsh in 1872. Both these remarkable types show reptilian affinities, in the possession of teeth, in the structure of the skull (though unhappily the palatal region is but imperfectly known), and in the pelvis; and Ichthyornis very notably in its slightly biconcave vertebræ, contrasting strongly with the saddle-shaped articulating surfaces of the vertebræ of modern birds. However strongly these genera suggest the idea of an evolutionary connection between reptiles and birds, their own place in the evolutionary series is not easy to determine. Ichthy-

ornis may be in or near the direct line of descent from Archæopteryx to some such generalized dromæognathous type as is represented by those curious living fossils, the Crypturi, from which divergent lines of evolution may have led, on the one hand, to the ostriches and other flightless Dromæognathæ, and, on the other hand, to the Carinatæ. Hesperornis, a degenerate and in some ways highly specialized form, stands certainly at the end of a side branch, and has left no descendants.

But the discoveries of most evolutionary significance, as already intimated, have been among the Tertiary mammals. A number of series have been traced, leading from generalized types in the Eocene, through forms of gradually increasing specialization, to genera which still survive. The first of these genetic series to be brought to notice was the genealogy of the horse, as traced by Marsh in 1874. Marsh's views were adopted by Huxley in his brilliant 'American Lectures,' and thereby gained a larger share of public attention than they would otherwise have received. Probably no single fact or group of facts brought to light since the appearance of the 'Origin of Species' has been so influential in bringing the theory of evolution into general acceptance. The genealogy of the horse has been corrected in detail and completed by later investigations. The line of descent may now be traced through Hyracotherium and Eohippus of lower Eocene, Protorohippus and Orohippus of middle Eocene, Epihippus of upper Eocene, Mesohippus of Oligocene, Anchitherium of lower Miocene, Parahippus, Protohippus, and Pliohippus of middle and upper Miocene, to Equus of Pliocene and Quaternary; while side branches lead to Hipparion, Hippidium, and other forms which have died without issue.

A similar series, though with not quite so fine gradations, reveals the genealogy of the camel. From the Eocene Protylopus this line is traced, through Oligocene Poebrotherium, to Pliocene Procamelus, whence one branch leads to Camelus, and the other to the South American Auchenia. It is indeed remarkable that the characteristically old-world types, Equus and Camelus, should have been evolved in North America and have become extinct in this their original home. Another series, beginning in the lower Eocene Systemodon, ends in the modern tapirs.

In like manner, among the very primitive carnivores which have been classified as the order Creodonta, the ancestors respectively of the dog and the cat have been recognized in the Eocene genera Vulpavus and Palæonictis.

Of extraordinary interest in an evolutionary point of view is the most primitive Tertiary fauna from the Puerco beds discovered by Cope in 1880. In that fauna is found the culmination of the Multituberculata, which made their first appearance in Triassic time, and whose teeth reveal their close relation to the Monotremata. But, with those survivals from Mesozoic time, appear generalized and primitive forms of placental mammals, wherein may be traced the ancestry of mammalian groups of later Tertiary and recent time. Hemiganus and Psittacotherium may be recognized as the ancestors of the Edentata. And, among the most primitive and generalized ungulates, the Phenacodontidæ, Protogonodon has been recognized as possibly the ancestor of the Artiodactyla, and Euprotogonia with more probability as the ancestor of the Perissodactyla.

In the American Museum, where some of our sessions are to be held, the evidence of these genetic series running through the Tertiary is placed before our eyes with incomparable fullness. As we behold the unparalleled richness of those collections, we

do not wonder that not even a call to be the official head of the army of science in this country could induce Professor Osborn to leave the museum which is at once the monument of his work in the past and the material for his work in the future.

But, however numerous are the gradational forms which have been brought to light, Darwin's principle of the imperfection of the geological record is in no wise superseded. It still remains true that the theory of evolution must stand or fall according to our judgment of the adequacy of that principle of Darwin. If the fossils accessible to observation and collected in our museums afford an approximately complete representation of the life that has existed in past ages, there is certainly no standing ground for any theory of evolution. But, while we have seen numerous chasms bridged by series of gradational forms, we have also come to a fuller appreciation of the significance of Darwin's principle of the imperfection of the geological record.

In the conclusion of Darwin's chapter on the subject, he used a striking illustration: "I look at the natural geological record as a history of the world imperfectly kept and written in a changing dialect. Of this history we possess the last volume alone, relating only to two or three countries. Of this volume, only here and there a short chapter has been preserved; and of each page, only here and there a few lines." In the light of our present knowledge of geological history, we are able to see that even this striking illustration fails to do full justice to the subject. The imperfection of the record consists not merely in the fact that some of the chapters are missing. It appears most strongly when we inquire just what chapters are missing. In the conception of continental history to which I have already referred, we have come to recognize a truth which, in somewhat distorted form, found expression in the old catastrophism. We have come to recognize that comparatively short periods of rapid geographical change alternate with long periods of relative stability or slowly progressive change. This is, in substance, the doctrine of critical periods as formulated by Le Conte. It is precisely in those critical periods that the record fails, and the gap is indicated by unconformability. Darwinians and Lamarckians alike must recognize that the periods of rapid geographical change must be the periods of most rapid change in fauna and flora. Evolutionary change must be directly or indirectly the result of a failure of adjustment between organism and environment.

The doctrine of critical periods has taken somewhat more definite form in Chamberlin's discussion of the effects of intermittent subsidence of the ocean bottom. The critical periods in geological history are the times when the rigidity of the crust yields to accumulating strain, when the ocean bottom subsides, when the continents emerge to larger area and higher altitude, when more or less of mountain-making takes place, and when the geographical changes bring in their train the diminution of the atmospheric supply of carbon dioxide and a tendency to cold and arid climates. Then come the long periods in which the continents are slowly denuded, the continental shelves are extended landward by encroachment of the sea and seaward by sedimentation, the quota of carbon dioxide is slowly replenished, and the fauna and flora which had been impoverished gradually expand to their former luxuriance. The chapters which are lost from the record are precisely the chapters which would contain the story of those critical periods, marked by extinction of manifold species, and by rapid change in adjustment to new and more rigorous conditions. The geological record of the progress of life is like a his-

tory of the United States, in which, among other less important chapters, the chapters on the Revolution and the Civil War are lost.

CONCLUSION

I have not attempted to give a history of geological investigation in this country. Of the great number of earnest and able investigators whose names illustrate the scientific history of this country—of those who have finished their work, but whose memory and influence can never die—of those still living whose achievements in the past are only the promise of greater work in the future—I have named but few, though many others are equally worthy. Of the men whose names I have mentioned, I have doubtless not in all cases mentioned the work which has been most meritorious or important. I have mentioned only those investigations which have a bearing on a few special subjects. Nor have I referred, except occasionally and incidentally, to the work of European students which has gone on parallel with that of students in our own country. American geologists have had no patent rights giving them a monopoly of any particular department of investigation. The limited time of such an address as the present renders impossible a critical discussion of the precise share in the study of the various subjects which belongs to American geologists. But I believe it may be fairly claimed that on the five subjects which I have discussed—the permanence of continents, the theory of mountain-making, the history of the Glacial period, the laws of subaerial denudation, the evolution of mammalian life—the work of American geologists has been relatively so important that the results deserve recognition as, *par excellence*, THE CONTRIBUTIONS OF AMERICA TO GEOLOGY.

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SCIENTIFIC BOOKS

The Development of Symbolic Logic: A Critical-Historical Study of the Logical Calculus. By A. T. SHEARMAN, M.A. London, Williams and Norgate. 1906.

As the subtitle indicates, the author has attempted a history of symbolic logic accompanied by a critical examination and estimate of the various systems as they may have contributed severally to the discipline from its earliest stages to the present time. He claims that in spite of the great variety of systems and methods there is clearly to be recognized but one logical calculus, and that the unity among the various symbolists abundantly compensates for the obvious differences. While this is true it should not be overlooked, however, that the progress of symbolic method has been retarded owing to the lack of a common symbolism such as we find in mathematics. The variety and the multiplicity of symbolical representation is, in my opinion, a serious defect. It is not merely that different writers are using different methods of symbolism—that in itself is sufficiently confusing—but also that any new operation is apt to give rise to some entirely new form of symbolism which might be represented equally as well by some new combination or new manipulation of the existing symbols already at hand. Within the scope of a few elementary symbols an indefinite range of differing processes and devices is possible, just as in mathematics the symbols used are exceedingly few—but they lend themselves easily and adequately to the exact expression of an innumerable array of operations and processes. The desideratum in a symbolic logic is, therefore, twofold: a common and a simplified symbolism. The simplicity of the symbolism of Leibniz, the founder of symbolic logic, is most striking; but the drift has been from this characteristic simplicity towards increasing difference and complexity. The author, by the way, does not give Leibniz his full due as the founder of the symbolic logic. Mr. Shearman insists that Boole is to have the complete credit of this on the ground that Boole worked independently and without any knowledge of the early work of Leibniz. The latter assumption seems

somewhat gratuitous, at least Mr. Shearman cites no authority for his statement. Be that as it may, it is true that Leibniz first suggested the problem of the symbolic logic, although he did not attain the solution of it. How often, however, the pioneers in a new field of thought have merely started inquiry without achieving the reward of discovery.

In reference to the problem of the symbolic logic which has been mentioned above, it might be a matter of general interest to the lay reader to learn somewhat more explicitly as to the characteristic features which it presents. The problem in the main is this: to devise a method by which any given relations expressible by symbols may be made to exhibit the full range of possibilities which these relations imply both affirmatively and negatively —that is what they render necessarily true or necessarily false. Moreover, there has been a constant endeavor through the whole development of symbolic logic to make such a method as general as possible. The generalizing of method, indeed, has been one of the chief characteristics of this development. For instance, the earlier symbolists dealt almost exclusively with logical classes; the later, as Frege, Peano, MacColl and others, extended their method so as to include propositions, and to represent every other relation as well as the ordinary relation of logical subsumption; the later symbolists also endeavor to embrace in their method the quantitative as well as the qualitative relations. In this connection I am constrained to refer to what may be called a fetich of symbolic logic and which has proved a snare to many. It is the notion that by representing certain ideas by symbols —the ideas themselves for a time being thus placed in the background—the merely formal processes of the accepted logical operations will disclose some entirely new relation of the symbols employed, which being reinterpreted in terms of the original ideas will reveal a new significance never before conceived. This is a vain delusion, for it labors under the misapprehension that there is something mysterious about certain formal processes by virtue of which new material content will be revealed. It has been urged that scientific discoveries of some moment may be stumbled upon merely by following out the subtle workings of formal processes whose significance can be appreciated only when such processes have been finally completed. It is as though the stream of reason was able to cut out certain subterranean channels to emerge again into the light of day. Leibniz had this idea as a kind of a will-o'-the-wisp, in his 'Characteristica Universalis' by which he thought that formal rules might take the place of brains and the conscious processes of thought. "If we had it," he says, "we should be able to reason in metaphysics and morals in much the same way as in geometry and analysis" (G. vii. 21). "If controversies were to arise, there would be no more need of disputation between two philosophers than between two accountants. For it would suffice to take their pencils in their hands, to sit down to their slates, and to say to each other (with a friend as witness, if they liked): Let us calculate" (G. vii. 200). The practical utility of the symbolic logic is not, however, in the direction of the discovery of new possibilities never before conceived, but rather in the line of providing a method by which every possibility is embraced in one comprehensive survey. In a field of complex relations it is very easy to overlook one and another of the many possibilities, and a method is valuable both theoretically and practically which provides that no single possibility can escape the attention, and which thus shows that logical implications are both manifold and complex. It is a question largely of whether every possibility has been brought to the attention of the observing mind, and not whether a possibility can be discovered by certain logical processes as an entirely new result never before imagined. Mr. Shearman cites as an example of a new truth discovered by calculation that of the existence of the planet Neptune by Adams and Leverrier. It must be remembered, however, that each one of these men in his calculation had clearly before him from the beginning the desired end which he expected his calculations to prove. In other words, these men were not working in the dark, simply trusting

themselves to a process which might be leading them any whither.

This volume contains an excellent historical sketch of the various systems of symbolic logic and as such is a most valuable book of reference. To read it with profit, however, some knowledge of the several systems is necessary. It would have been a manifest advantage had the author given, for instance in his chapter on The Process of Solution, a more detailed and elementary account of the original method of Boole, or of the method of Venn, being as it is a developed form of the Boole method. Thereby the difficulties for the lay reader would have been overcome to a great extent.

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Thought and Things, or Genetic Logic. Vol. I. *Functional Logic.* JAMES MARK BALDWIN. London, Swan, Sonnenschein & Co.; New York, The Macmillan Co., 1906. Pp. xiv + 273.

This is the first of three volumes, appearing in both French and English, on a subject never before given comprehensive systematic treatment. It brings into use a somewhat unusual terminology. The terms pragmatelic, semble, sembling, autotelic, heterotelic, syntelic, psychonomic, autonomic, heteronomic, syndoxic, progression, mode, schema are used with restricted although clearly defined meanings, and we might add to the list. Some readers will wish for more elucidation and continuity in places and there are some passages whose meanings are rather elusive. But the methodological difficulties of the subject are unusually great and have been handled with a remarkable degree of success. The author's evident interest in the subject itself, rather than in the style of the discussion, is neither surprising nor reprehensible.

The author realizes that the title, Genetic Logic, is likely to provoke criticism (pp. vii, 18). We should say the place of genetic logic among the philosophical disciplines is not unlike that of sociology among the social sciences. Sociology is neither history, economics, psychology nor anthropology and some whose contributions to these subjects give them a right to speak say there is no separate science of sociology. Genetic logic is neither straight logic, straight psychology, nor straight epistemology. Logic is not genetic, psychology is not interested in questions of logical validity, and epistemology, although broader and more elastic than the other two, does not involve as much psychology as this book presents. And yet the author is correct in assuming that the problems here discussed are real and pressing. They are not new. The time seems to have come for a systematic presentation of all this material and it is to be hoped that this is only the first of several works on the subject. It is a colossal task for which the philosophical *Zeitgeist* has been long in training, and one can only praise the keenness and comprehensiveness of this treatment.

The author confines himself to the same rules of observation and hypothesis as those observed in 'the empirical sciences generally' (p. 9), asking the questions—How? and Why?—as well as the question—What?—with reference to each form and mode of knowledge. Neither the formal logician's logic, nor the metaphysician's 'logicism,' will concern us here, but rather the knower's logic, cognitive processes viewed from the knower's standpoint. We are to study the genesis of knowledge and thought, and construct a genetic theory of reality. The former topic is discussed in the first two volumes, the latter, in volume third. The first volume presents a genetic theory of what the author calls the pre-logical cognitive functions, the second, a genetic theory of thought and judgment, and the third, a genetic theory of real logic or the 'hyper-logical functions' (p. 15). 'Genetic psychology of cognition' and 'genetic epistemology' (p. 18) are other names for the two main topics of the work. It seems to us that 'genetic epistemology' would be a good title for the entire work.

The author asks (1) 'what are the conditions determining the construction of objects at any given stage of mental development, and (2) what are the psychic characters of the objects thus determined' (p. 30). He distinguishes "in the actual results to which the research has led, the following phases of con-

sciousness traced in each case along with the objects through a series of modes: (1) The controlling conditions of the determination (that is, the control of the object); (2) the motive to each of the determinations (the problem of interest as 'practical,' or 'theoretical,' or other); (3) the function involved in each determination; (4) the meaning of the object over and above its actual objective marks' (p. 30 f.). On page 34 seven sorts of objects are distinguished and defined as follows. (1) The projective object of sense, (2) the image object (of memory and fancy), (3) the make-believe object ('the first determination of the semblant object'), (4) the substantive object (either mind or body), (5) object of experience (object to a subject), (6) judged or logical object (an object of experience which the psychic subject as such is aware that it is in some sense acknowledging or controlling), (7) the esthetic object (of higher semblance in which the dualism of inner and outer controls is annulled in a state of immediate contemplation). Corresponding to these are seven modes, namely, sense, image, play, substantive, subject, logical, æsthetic (p. 32).

A criticism suggests itself here. This is a work on the knower's logic, but the first four 'objects' in this list are not objects to the knower. They are objects only to the psychological observer who analyzes. They are (to use words which the author uses to characterize a different subject) 'abstract meanings of our reflection, that is meanings only to a consciousness that can have an object that means this.' This suggests another fundamental objection, namely, to the author's treatment of mere presence in consciousness as knowledge, 'projective cognition.' This treatment leads to setting up a 'subject,' or a 'consciousness,' or etc., as the other term of the cognitive relation, and gives us an epistemological dualism for which there is no solution.

This epistemological dualism runs through the entire book and is especially evident in Chapter III., on How Knowledge is Made. Here a system of 'grasping' and 'habitual dispositional processes' is set over against an extra-psychic stimulus which, in some cases, 'is not content with knocking down our fortifications,' but 'rides full-armed through our walls, and compels its recognition in certain of its characters *for what it is*' (p. 50, foot-note). This 'outside world' whose impacts (p. 54) are represented in consciousness by the 'sense residuum or datum' is as much a thing-in-itself as Locke's outside world or the explanation of our modifications of sensibility in Kant's epistemology. If the author means that this dualism of controls is merely psychological, the uncritical dualism of naïve consciousness and one which he does not regard as real or valid, we should say it is a needlessly artificial way of describing the content of consciousness, and also that it is misleading to cite and criticize in this connection an epistemological doctrine of control such as that of Dewey.

In Chapters VII., VIII. and IX., on Meaning, the limiting and negative sense datum is defined as a 'meaning,' and on page 172, 'meanings arise as variations which presented complexes take on for the satisfaction of varying dispositions.' But how is this to be reconciled with the doctrine of a 'foreign' sense-datum, and with the general doctrine of 'outer control'? At the conclusion of the book the author claims to have found that there is no sort of discontinuity or dualism between prelogical function and thought, and that the positive dualism is one within the operation of the developing function of cognition, the dualism of meanings (p. 272). A consummation devoutly to be wished at the present, but what shall be said of the foreign warrior who 'rides full-armed through our walls'?

In a foot-note on page 50 the author seems to ascribe to Dewey an epistemological dualism of which we understand the latter to be the sworn enemy. We may be in error as to the teachings of both men, but so far as they discuss the same aspects of the subject at all we find many points of similarity between the author's doctrine of control (and also his outlined experimental logic) and the teachings of Dewey. The latter holds that *to the knower* control always means objectivity, while Bald-

win teaches a dualism of subjective and outer controls; but in other respects they are not so far apart.

Play, sembling and experimentation are central in this theory of knowledge. In play we semble, that is, we treat an object which we have invented, one 'freely' determined by 'subjective control,' as though it possessed certain coefficients of reality which it lacks. By experimentation we test these play constructions and find that they are either mere fancies (belonging to the inner world of subjective control) or else sense objects (possessing universality and belonging to the outer world of foreign control). Thus play and experimentation, leading to judgment, mediate between the 'inner' and the 'outer,' between 'subjective control' and 'outer control.' The author does not refer to language, or to sympathy, imitation, jealousy, bashfulness, gregariousness and other instinctive or impulsive reactions which involve social situations. Why should play be singled out as the only impulsive reaction contributing to the development of judgment? Universality is not involved in mere play because (1) few can enter into a game and (2) both the objects and the self of play are tentative and fictitious. Mere semblance is not characteristic of objects of knowledge as such. In short, we find a gap between play and experimentation, between sembling and judging, which the book has not filled, the gap between perception and conception, between sense and reason, between mere sentience and reflection. The author's theory seems to lead to the doctrine that facts are all 'outer'—that they are ultimately trans-objective—while meanings and values are all subjective (see pp. 135 f.), and judgment must perform the miracle of joining them. Beyond this difficulty, the author's dualism of subjective and outer controls would make genuine experimentation and judging impossible. These brief critical suggestions, of course, need elaboration, but the reviewer's respect for the writer's results as well as his sense of the importance of this discussion incline him to let them stand.

G. A. TAWNEY

THE WAYS OF SHEEP.

The Flock. By MARY AUSTIN. Pp. 266, illustrated. Houghton, Mifflin and Company. 1906.

Scientific observation as conducted by scientific men is rigorous, repeated and checked by the ingenious employment of experimental control of conditions. Observation by the nature lover may not be so guarded and tested. And every publication by word of mouth or impress of type of obviously mistaken record of seeing or of misinterpretation of the really seen that comes from the nature lover confirms the rigorous-minded scientific man in the belief that only his sort of observation reveals the truth. Hence we do not search literary books for contributions to science: which is a habit of omission that may lose to us some valuable data.

Mary Austin, an author known especially to readers of the *Atlantic Monthly* and to those generally who seek to acquaint themselves with the better sort of American writing, has included in 'The Flock' a host of singularly interesting and suggestive observations on the ways of sheep. The author has lived near (in more ways than one) sheep and sheep dogs and sheep men for seventeen years, and is a keen and careful observer and an honest and gifted recorder of her observations. Hence 'The Flock' is a book which the driven scientific man may read for recreation and information at once. How unusual!

I shall take space to refer to but two or three of Mrs. Austin's observations or summations of observation. The 'mob mind' of sheep is a very real thing in determining the ways of the flock. In the flock

> there are always leaders, middlers and tailers, each insisting on its own place in the order of going. Should the flock be rounded up suddenly in alarm it mills within itself until these have come to their own places.
>
> Suppose the sheep to scatter widely on a heather-planted headland, the leader feeding far to windward. Comes a cougar sneaking up the trail between the rooted boulders toward the meanest of the flock. The smell of him, the play of light on his sleek flanks startles the unslumbering fear in the meanest; it runs widening in the

flock-mind, exploding instantly in the impulse of flight.

Danger! flashes the flock-mind, and in danger the indispensable thing is to run, not to wait until the leader sniffs the tainted wind and signals it; not for each and singly to put the occasion to the proof; but to run—of this the flock-mind apprizes—and to keep on running until the impulse dies faintly as water-rings on the surface of a mantling pond. In the wild pastures flight is the only succor, and since to cry out is to interfere with that business and draw on the calamity, a flock in extremity never cries out.

Consider, then, the inadequacy of the flock-mind. A hand-fed leader may learn to call the herder vociferously, a cosset lamb in trouble comes blatting to his heels, but the flock has no voice other than the deep-mouthed pealings hung about the leader's neck. In all that darkling lapse of time since herders began to sleep with their weapons, affording a protection that the flock-mind never learns to invite, they have found no better trick than to be still and run foolishly. For the flock-mind moves only in the direction of the original intention. When at shearings or markings they run the yearlings through a gate for counting, the rate of going accelerates until the sheep pass too rapidly for numbering. Then the shepherd thrusts his staff across the opening, forcing the next sheep to jump, and the next, and the next, until, Jump! says the flock-mind. Then he withdraws the staff, and the sheep go on jumping until the impulse dies as the dying peal of the bells.

Have sheep inherited acquired characters of habit or increased in this way their mental equipment? Not according to what has just been written. But consider this:

I do not know very well what to make of that trait of lost sheep to seek rock shelter at the base of cliffs, for it suits with no characteristic of his wild brethren. But if an estray in his persistent journey up toward the high places arrives at the foot of a tall precipice, there he stays, seeking not to go around it, feeding out perhaps and returning to it, but if frightened by prowlers, huddling there to starve. Could it be the survival, not of a wild instinct—it is too foolish to have been that—but of the cave-dwelling time when man protected him in his stone shelters or in pens built against the base of a cliff, as we see the herder yet for greater convenience build rude corrals of piled boulders at the foot of an overhanging or insurmountable rocky wall? It is yet to be shown how long man halted in the period of stone dwelling and the sheep with him; but if it be assented that we have brought some traces of that life forward with us, might not also the sheep?

But, from the other side, consider this:

Where the wild strain most persists is in the bedding habits of the flock. Still they take for choice, the brow of a rising hill, turning outward toward the largest view; and never have I seen the flock all lie down at one time. Always as if by prearrangement some will stand, and upon their surrendering the watch others will rise in their places headed to sniff the tainted wind and scan the rim of the world. Like a thing palpable one sees the racial obligation pass through the bedded flock; as the tired watcher folds his knees under him and lies down, it passes like a sigh. By some mysterious selection, it leaves a hundred ruminating in quietude and troubles the appointed one. One sees in the shaking of his sides a hint of struggle against the hereditary and so unnecessary instinct, but sighing he gets upon his feet. By noon or night the flock instinct never sleeps. Waking and falling asleep, waking and spying on the flock, no chance discovers the watchers failing, even though they doze upon their feet; and by nothing so much is the want of interrelation of the herder and the flock betrayed, for watching is the trained accomplishment of dogs.

Our 'amelioration' of the sheep has certainly lost them much—even though we have gained. This is, of course, the familiar story of artificial selection. It is chiefly artificial degradation. Mrs. Austin records this:

Of the native instincts for finding water and knowing when food is good for them herded goats have retained much, but sheep not a whit. In the open San Joaquin, said a good shepherd of that country, when the wind blew off the broad lake, his sheep, being thirsty, would break and run as much as a mile or two in that direction; but it seems that the alkaline dust of the desert range must have diminished the keenness of smell, for Sanger told me how, on his long drive, when his sheep had come forty miles without drink and were then so near a water-hole that the horses scented it and pricked up their ears, the flock became unmanageable from thirst and broke back to the place where they had last drunk.

And this:

Sheep will die rather than drink water which does not please them, and die drinking water with

which they should not be pleased. Nor can they discriminate in the matter of poisonous herbs. In the northerly Sierras they perish yearly, cropping the azaleas; Julien lost three or four hundred when wild tobacco (*Nicotiana attenuata*) sprang up after a season of flood water below Coyote Holes; and in places about the high mountains there are certain isolated meadows wherein some herb unidentified by sheepmen works disaster to the ignorant or too confiding herder. Such places come to be known as poison meadows, and grasses ripen in them uncropped year after year.

What have the sheep come to know of man in their fifty centuries' association with him? Mrs. Austin answers:

It is doubtful if the herder is anything more to the flock than an incident of the range, except as a giver of salt, for the only cry they make to him is the salt cry. When the natural craving is at the point of urgency they circle about his camp or his cabin, leaving off feeding for that business; and nothing else offering, they will continue this headlong circling about a boulder or any object bulking large in their immediate neighborhood remotely resembling the appurtenances of man, as if they had learned nothing since they were free to find licks for themselves, except that salt comes by bestowal and in conjunction with the vaguely indeterminate lumps of matter that associate with man. As if in fifty centuries of man-herding they had made but one step out of the terrible isolation of brute species, an isolation impenetrable except by fear to every other brute, but now admitting the fact without knowledge, of the God of the Salt. Accustomed to receiving this miracle on open boulders, when the craving is strong upon them they seek such as these to run about, vociferating, as if they said, In such a place our God has been wont to bless us; come now let us greatly entreat Him. This one quavering bleat, unmistakable to the sheepman even at a distance, is the only new note in the sheep's vocabulary, and the only one which passes with intention from himself to man. As for the call of distress which a leader raised by hand may make to his master, it is not new, is not common to flock usage, and is swamped utterly in the obsession of the flock-mind.

Then there are the sheep dogs, those wolves that we have ameliorated to protect the sheep from other wolves. But our space prevents even a tasting of the interesting notes on dog ways that the book offers. Let us but just note the strong insistence of our author observer that dogs are not bred sheep dogs, but trained to be sheep dogs. "What good breeding means in a young collie is not that he is fit to herd sheep, but that he is fit to be trained to it." Rather against the inheritance of acquirements, this.

There is much keen observation, much shrewd suggestion, and no end of delight in 'The Flock.' And trained in the scientific method or not, Mrs. Austin is honest and truthful as one may be. That is, she tells only what to her eye and ear and mind comes with the seeming of truth. No rigorous scientific pundit can do more. For truth is for any of us too often at the bottom of the well.

VERNON L. KELLOGG
STANFORD UNIVERSITY,
CALIFORNIA

SOCIETIES AND ACADEMIES

THE GEOLOGICAL SOCIETY OF WASHINGTON

THE 185th meeting of the society was held on January 9, 1907, with President Lindgren in the chair and fifty-four persons present.

Regular Program

The Paleozoic Section of the Upper Yukon, Alaska: ALFRED H. BROOKS and E. M. KINDLE.

This paper was presented by Mr. Brooks, who, in company with Mr. Kindle, devoted part of the field season of 1906 to a detailed study of the rocks exposed between the international boundary and Fort Yukon, along the banks of the Yukon and Porcupine Rivers.

The total thickness of Paleozoic strata, comprising this section, is estimated to exceed 15,000 feet, but as the bottom was not determined, it may be very much greater. *The lowest member* of the succession is a series of quartzites with intercalated limestones and shales, which is well developed on the Porcupine near the international boundary. Provisionally, at least, these rocks may be correlated with the more highly altered rocks called the Birch Creek schists which occur in large areas south of the Yukon. On the Porcupine these rocks are comparatively little altered and intrusives appear to be entirely absent, except for occasional small dikes, while

to the south the same series is much metamorphosed, sometimes to complete recrystallization, and intrusives are abundant. *The next member* is made up of limestone which on the Porcupine is about 600 feet thick and carries Ordovician fossils. This terrane comprises bluish-gray, non-magnesian limestones, distinguishable lithologically from the buff dolomitic series which follows them, but has not been definitely recognized in other parts of the province. The relation of these Ordovician beds to the older rocks is probably one of unconformity.

The Silurian is represented by at least 2,500 feet of magnesian limestone with some quartzites and slates, which are probably conformable to the Ordovician. Its correlation with the Fortymile series of Spurr is suggested but not proven. When *the Devonian* is considered, the problem becomes more complex, as no complete section of these rocks has yet been found. In general terms it can be said to be made up of shales, slates and cherts, with some limestones, while locally ancient lava flows and tuffs dominate over the sediments. On the Yukon the igneous rocks are very much more abundant than the sediments, while on the Porcupine the sediments are most abundant. The thickness is much in doubt, but can not be less than 3,000 feet and may be twice as great. The relation of the Devonian to the Silurian is unknown, but is probably one of unconformity. A black shale and slate bed some 1,300 feet thick constitutes the upper member of the Devonian and within this formation is probably the dividing line between the Devonian and the Carboniferous.

In this province the Carboniferous embraces about 4,000 feet of sediment, usually characterized by an absence of igneous rock. Overlying the shale mentioned above is about 1,000 feet of a thin-bedded limestone and shale series which carries Carboniferous fossils which have not yet been studied in detail. After these limestones and shales were laid down, the character of the sediments changes abruptly, indicating possibly a period of erosion. A heavy bed of chert conglomerate forms the basal member of the higher series and this is succeeded by shales, sandstones and conglomerates with an aggregate thickness of 1,600 feet. A limestone bed some 200 feet thick constitutes the youngest member of the Carboniferous. This is overlaid in different parts of the province by younger terranes of various ages. On the Porcupine the lowest member of the Devonian is a limestone 300 feet thick, while olive and chocolate-colored shales, in part fossiliferous, appear to comprise a large share of the Middle Devonian sediments.

The Stratigraphic Succession in the Region Northeast of Cook Inlet, Alaska: Sidney Paige and Adolf Knopf.

In this communication the combined results of the field studies of this region in 1906 were presented by Mr. Paige. The rocks exposed in this geologic section were found to range in character from garnetiferous mica schists of probable pre-Silurian age to unconsolidated Pleistocene stream and glacial gravels. Jurassic strata are very extensively developed and are divided into two unconformable formations, the lower of which consists chiefly of andesitic breccias and amygdaloids, 1,000 feet in thickness and overlain by graywackes, shales, sandstones and conglomerates. Marine shells, found both in the tuffs and in the graywackes, fix its age as Lower Middle Jurassic. The younger Jurassic rocks consist of shales, arkose, sandstones, conglomerates with some interstratified tuffs. The age indicated by fossil evidence ranges from the upper part of the Middle Jurassic through the Upper Jurassic. Conformably overlying these rocks is a Lower Cretaceous limestone 300 feet thick.

The Upper Eocene (Kenai) rocks of this section are freshwater sediments, consisting of shales, sandstones and conglomerates folded and carrying workable coal beds, of a maximum thickness of 17 feet. Their total thickness is not less than 3,000 feet; their age was determined on the fossil flora collected. Basaltic lavas and tuffs overlie the older rocks. They attain a thickness of 1,000 feet and form the summit topography of the region.

The Volcano Aso, in Kiushiu, Japan: ROBERT ANDERSON.

The volcano Aso-san is situated in the central part of the island Kiushiu in southern Japan. It has an active, modern crater a mile and a half in circumference which has been in eruption frequently during the past century, but its chief geological interest lies in the fact that it has an ancient crater ten or eleven by fourteen miles in diameter, larger than any other known crater. The crater basin, occupying an area of about one hundred square miles, is contained within sheer walls of andesite that rise on the average to a height of two thousand feet above the floor. A range of volcanic mountains, probably of subsequent origin, extends diametrically across the old crater and rises out of it over four thousand feet to a maximum altitude of five thousand six hundred feet above the sea. The vast crater of Aso-san is thought to have been formed either by the blowing off of the overlying mountain mass, or by the subsidence to a depth of three thousand feet or more of the oval region now occupied by the great bowl.

FRED E. WRIGHT,
Secretary

THE PHILOSOPHICAL SOCIETY OF WASHINGTON

THE 623d meeting was held on November 24, 1906, President Abbe in the chair.

Mr. Bigelow presented a request from a central committee in Geneva that this society approve and use in its publications the auxiliary language *Esperanto.*

Mr. Cyrus Adler presented a very interesting and felicitous memorial address on the late Professor Samuel P. Langley; his most notable services were, the establishment of uniform time, the study of the sun and its infra-red spectrum, and the researches in aerodromics, which for the first time 'made the subject respectable.'

Mr. F. W. Clarke spoke on 'The Quadri-Centennial at Aberdeen,' at which he represented the Smithsonian Institution, describing briefly the magnificent new buildings and elaborate ceremonials.

Mr. E. Buckingham then discussed 'The Thermodynamic Scale of Temperature.' Since temperatures are not quantities in the ordinary sense of the word and are not additive, a scale of temperatures is an arbitrary thing and scales founded on the properties of different materials will not agree. In attempting to reconcile the readings of different gas-thermometers and reduce them to a theoretical thermodynamic scale, reliance has been placed on the observations on the change in temperature of gases forced through a porous plug, and excessive extrapolation has been necessary. The speaker has attempted to coordinate the discordant results by applying the 'law of corresponding states' and finds the discrepancies are very much reduced.

THE 624th meeting was held on December 8, 1906, Vice-President Bauer in the chair.

The evening was devoted to the annual address of the president, Professor Cleveland Abbe. His subject was 'The Progress of Science as exemplified by Meteorology.' He gave a rapid sketch of the history of the science with references to the Americans who had contributed to its advancement, and in conclusion exhibited a number of lantern-views of a water-spout photographed by numerous observers last summer near Marthas Vineyard, Mass., and thoroughly studied by Professor Bigelow.

THE 625th meeting, the 36th annual meeting, was held on December 22, 1906.

The report of the secretaries showed a resident membership of 130; fifteen regular meetings have been held; Vol. XIV. of the *Bulletin* has been completed and distributed.

The treasurer's report showed a sound financial condition.

The following officers were elected for 1907:

President—John F. Hayford of the Coast and Geodetic Survey.

Vice-Presidents—L. A. Bauer, A. L. Day, E. B. Rosa, C. K. Wead.

Treasurer—B. R. Green.

Secretaries—G. K. Burgess, R. L. Faris.

General Committee—C. G. Abbot, C. Adler, L. J. Briggs, W. A. DeCaindry, W. S. Eichelberger, L. A. Fischer, R. A. Harris, J. Page, I. Winston.

CHARLES K. WEAD,
Secretary

THE NEW YORK ACADEMY OF SCIENCES
SECTION OF GEOLOGY AND MINERALOGY

At the meeting of December 10, 1906, the following papers were presented:

Present Structural Character and Probable Former Extent of the Palisade Trap: Alexis A. Julien.

In the Palisades, the trap sheet along the lower Hudson River, the structures most generally known are the vertical or columnar and the coarsely bedded structure with foliation parallel to that of the whole stratum. Less familiar is a coarse concentric structure visible on horizontal surfaces. A thinly lamellar structure has also been brought out by natural etching on weathered vertical surfaces, which appears to represent a flow structure, or, perhaps more properly, a pressure lamination.

At a certain zone along the face of the escarpment an interrupted or continuous sheet of decayed rock crops out, a principal cause of destruction of the columns by undermining. It indicates a process of preglacial decay of extreme antiquity, and is attributed to a balance of conditions of perpetual moisture in a coarsely granulated band of the trap. Two systems of faults traverse the trap sheet from north, or a little east of north, to the opposite point, and from north-northwest to south-southeast. Eight or nine faults have been located by previous observers, and some of these extend for several miles. But there is abundant evidence of a large number of other faults, those of one system marked by depressions which furrow the top of the ridge in the direction of its trend; those of the other system indicated by nicks or clefts (cloves) along the edge of the escarpment. The original thickness of the trap sheet shows that these hollows represent the bottoms of ancient gorges of a drainage system guided by the faults over the summit of the ridge. This signifies an enormous amount of denudation, doubtless effected by the continental glacier, which, in connection with the faults, has also resulted in the southwestward slope of the ridge down to the sea-level. As to the mooted question of the eastward extension of this sheet of the Newark formation beyond the Hudson River, direct evidence appears in the low rock terraces between Dobb's Ferry and Peekskill, formerly occupied by the overlap of this formation; in the common distribution of zeolites, etc., in cracks of the gneisses on the east side of the river, which could only have been derived through infiltration of thermal waters from an overlying trap sheet; and in the correlation of depressions on opposite sides of the river, which have been impressed by an ancient drainage system over the uplifted Mesozoic terrane and across the present line of the Hudson River.

Development of the Inner Wall in Paleozoic Corals: G. E. Anderson.

From serial sections of *Craspedophyllum subcæspitosum* it was shown that the primitive union of septa is retained even after radial condition of septa is reached. The wall is formed by the turned-over and fused ends of the septa and at one stage resembles the fossular wall of certain corals. Final closure of the wall by bridging of the fossular gap occurs in accelerated mutations of this type and is normal in *Eridophyllum*. No true inner wall exists in *Acervularia* and similar genera.

The Geographical Classification of Marine Life Districts: A. W. Grabau.

After a discussion of principles the following tentative classification was outlined:

GEOGRAPHIC DIVISIONS	BATHYMETRIC LIFE DISTRICTS PRESENT
I. Intercontinental seas or oceans.	Littoral, pelagic, abyssopelagic, abyssal.
II. Intracontinental seas.	
a. Mediterraneans.	Littoral, pelagic, abyssopelagic, abyssal.
b. Epicontinental seas.	Littoral, pelagic.
III. Continental seas or lakes.	Littoral, pelagic, more rarely abyssal.

The speaker advocated that the term littoral be extended so as to cover all that district from high water to the edge of the continental shelf (or beyond, to the point where the photic or lighted portion of the sea bottom ends), instead of restricting it to the shore zone, as is often done. He also advocated the restriction of the term epicontinental sea to that type of intracontinental sea which has no abyssal district. The advantage in pre-

cision thus gained and the natural character of the classification proposed were pointed out.

A. W. GRABAU,
Secretary of Section

THE AMERICAN CHEMICAL SOCIETY. NEW YORK SECTION

THE third regular meeting of the season of 1906–7 was held at the Chemists' Club, 108 W. 55th Street, on January 11.

The Nichols medal, awarded annually for the best paper read before the New York Section, was presented to Howard B. Bishop for his paper 'On the Estimation of Minute Quantities of Arsenic.' Favorable mention was made of the paper of E. H. Miller and J. F. Thompson on the 'Silver Platinum Alloys' and of the papers of F. B. Power and Frank Tutin on the 'Chemical Examination of *Æthusa Cynapium*' and on the 'Chemical and Physiological Examination of *Chailletia Toxicaria*.'

The rest of the evening was devoted to a symposium on the pure-food law by Messrs. H. W. Wiley (address read by chairman), Virgil Coblentz, R. Z. Doolittle and M. D. Foster. Further discussion followed, in which Messrs. Wm. J. Schieffelin, Albert Plaut, J. B. F. Herreshoff and L. L. Watters took part.

C. M. JOYCE,
Secretary

DISCUSSION AND CORRESPONDENCE

FACTS AND INTERPRETATIONS IN THE MUTATION THEORY

THE foremost champion of de Vries's mutation theory in this country undoubtedly is Dr. D. T. MacDougal, and he has largely contributed to the popularity of this theory. In a recent article[1] he takes up certain objections made by various writers, and attempts to show that they are without foundations or opposed to the known facts. But the criticism of the objections made by C. H. Merriam, D. S. Jordan and the present writer fails to convince, and only serves to demonstrate that the vital points have been misunderstood.

[1] 'Discontinuous Variation in Pedigree-Culture,' *Pop. Sci. Monthly*, 69, Sept., 1906, pp. 207–225.

Before I try again to give a review of my objections to de Vries's theory, I shall prove in detail that MacDougal's criticism of them, as well as of those of Merriam and Jordan, is unsatisfactory. It may appear as presumptuous, when I take it upon me to talk in behalf of the latter two gentlemen, who are well able to take care of themselves,[2] but I may be excused on the ground that I hold precisely the same views, and am thus defending my own opinions.

I. MacDougal first takes up Merriam's contention, that the study of geographical distribution of animals shows no evidence of 'mutation' (in the sense of saltation or discontinuous variation), since there are gradual transitions, which point to a progressive development of minute variations. This is not admitted by MacDougal, because he maintains (p. 209) that 'once a mutant has appeared, no evidence of its distribution can be taken to account conclusively for its origin.' Jordan has answered this in the article just referred to. But there is yet another aspect. Merriam did not express any view as to the *origin of mutation* (*saltation*); he only wanted to bring out the fact that mutations, in the sense of discontinuous variations, seem to be extremely rare in nature, which is indicated by the fact that, morphologically, varieties and even species are often very close to each other, and that, if there are cases where a discontinuity is apparent, a closer investigation of the distribution, not only of the supposed mutant, as MacDougal puts it, but of the mutant and its allied forms, reveals the existence of intermediate forms.

With reference to this latter case, I should like to make a few additional remarks. Granted the existence of a connecting form between two extremes, which appear to fulfill the morphological requirements of mutation,[3]

[2] See Jordan's rejoinder in SCIENCE, September 28, 1906, p. 399.

[3] We always are to remember that, strictly speaking, there is no morphological difference between fluctuating variation and mutation; the latter can only be recognized by experiment, according to de Vries, and also MacDougal. Thus it is not correct to talk, as MacDougal does, of

the question is, whether this connecting form represents a stage of development from one extreme to the other, or, as the de Vries school supposes, a hybrid between the extremes. This question, in my opinion, may be settled in many, if not in all, cases, by the facts of the geographical distribution. If the overlapping area of the ranges of the two extremes is occupied by an intermediate form associated with the two original forms, hybridization is indicated; if, on the contrary, in the intermediate area the intermediate form is exclusively represented, the latter very likely marks a connecting step in the development from one to the other extreme.

I am much tempted to illustrate this here by an example I have discovered in the distribution of certain species of river crawfishes (group of *Cambarus propinquus*). But it would take up too much space to give all the facts. It will be presented to the public in due time, in fact, the paper is just now going through the press. Suffice it to say that both of the above cases are illustrated in this example, but that the final decision was possible only after a thorough investigation of the geographical distribution of these forms had been made, by researches covering the whole of the state of Pennsylvania, and parts of Maryland, West Virginia and Ohio.

MacDougal further says (p. 209) that "a number of zoologists have assumed to speak of the distribution of plants, with apparently no basis except 'general information' to the effect that closely related species do not have the same habitat. This has been variously put, but the general meaning is as given." On page 210 he talks of this as 'the idea * * * that closely related species do not occupy the same region.' I believe I am included in this number of zoologists, although I never expressed such an idea; but since I do not know of any other zoologist who did, I think MacDougal refers to something similar I have said. I expressed it thus:[4] 'two closely allied species never occupy absolutely the same *range* under identical ecological conditions.' This, however, does not exclude the possibility that *parts* of their different ranges may overlap, and that in certain regions they may be found together, and, moreover, a case in point has been described by me.[5] Thus it is evident that MacDougal has misunderstood me, and that he battles against a fancied idea, which, indeed, is disproved by the instance given (*Opuntia fulgida* and *mammillata*), and to which I am able to add numerous other examples of plants[6] as well as animals. But this does not influence my contention, *that closely allied species of plants or animals never possess precisely the same geographical range.*

With reference to Jordan's opinion that *Œnothera lamarckiana* might be a hybrid, which is also held by the present writer, MacDougal thinks that it possibly might be a good, natural species. This question, however, is not essential for the interpretation of de Vries's experiments. The suggestion that it might be a hybrid, or the fact that, in Europe, it is an escaped garden-form, is advanced only to explain the remarkable variability of it. De Vries, assuming that it is as good as a natural species, tries to account for its wonderful capacity to throw off mutants, which is not generally found among natural species, by believing that there may exist, in any species, a time or period of especially vigorous and frequent mutation. But con-

mutation as a synonym of discontinuous variation. A mutation may be due to discontinuous variation, and, as de Vries declares, generally is, but not always. In this respect, MacDougal, in the article referred to, certainly goes beyond de Vries. If this is borne in mind, it is clearly seen that Merriam's objection does not affect at all de Vries's theory as a whole, but only the part of it that says that discontinuous variation or saltation is a necessary or frequent attribute of mutation (in the sense of creating the faculty to become a true breeding form).

[4] Science, June 22, 1906, p. 949.

[5] Science, March 30, 1906, p. 504.

[6] For instance, *Orchis ustulata* and *tridentata*, two closely allied species, but of quite different aspect, grow side by side (forming hybrids) upon the meadows of the valley of the river Saale, near Jena, Germany; on a certain hillside near Jena, *Ophrys muscifera* and *aranifera* grow together (also forming a hybrid).

sidering the fact that, in Europe, *Œnothera lamarckiana* is surely an escaped garden-form, and that it actually has been there under the care of florists for a long time, the possibility that it might be found in the wild state in America does not affect in the least degree the very tempting assumption that its strange behavior is due to cultivation with all its inherent and accessory incidents, exactly as is the case in other garden-forms.

MacDougal regards it as a specimen of 'literary license' and 'inaccuracy' (p. 213), when I say that de Vries entirely failed to take notice of the principle that the discovery of intermediate forms serves to show that two supposed true breeding forms do not possess the rank of species as understood by the taxonomist, and that he also failed to show that his so-called elementary species are not connected by intermediate forms. I do not see where the 'license' and 'inaccuracy' come in, for it is *a fact* that de Vries nowhere reported that he went over the whole area of any of the species used or referred to by him, and tried to ascertain whether there are anywhere such forms. Indeed, he reports that on rare occasions he found something which might be taken for connecting forms, but he never searched carefully and conscientiously for them. On the other hand, I know positively from my own experience that such forms do exist at least in some of the elementary species discussed by de Vries: I found them myself in nature in the group of *Viola tricolor* and *lutea*, and saw them in the case of *Draba verna* in de Bary's laboratory.[7]

As regards my 'estimate of the futility of experimental methods' and my 'mistrust' of them, MacDougal (p. 213) has not understood my standpoint. I have never said anything that might be construed as if I 'mistrusted' experiments or believed them to be 'futile,' on the contrary, I fully agreed that experiments ought to be made, but warned against too great complexity and improper interpretation.[8] I chiefly called attention to the complexity of conditions offered in cultures in the botanical garden, which is met by MacDougal (p. 213) by the statement that it is not the case, that 'domesticated races' have resulted from the 'effects of tillage.' But, disregarding the fact that 'tillage' is only one of the many factors contributing to the peculiar features of environment in the garden, I never said that the effect of tillage (or any other environmental factor) is the production of 'domesticated races.' I attribute to the environment the power to influence 'variation,' but in order to obtain 'domesticated races,' that is to say, forms which breed true, I always insisted that pedigree-culture is necessary. In nature the analogous process, selection and segregation, leads to the formation of species.

II. It is possibly well to present here again my objections to de Vries's mutation theory, and, to further the correct understanding of my views, I shall try to represent the matter in a somewhat different form, emphasizing chiefly what are the undoubted facts, and what are their interpretations on the part of de Vries and on my part.

My first and fundamental contentions are:

1. *De Vries's conception of 'elementary species' is inadequate.* There are, indeed, forms in nature which have a tendency to breed true, but which are not isolated from other forms, but these forms should not be called species. They have been called, for instance, by Darwin,[9] 'varieties,' and are distinguished by this quality from 'variations.' On the other hand, there are in nature true 'species,' characterized by the fact that the tendency to breed true is fully developed, and that there are no connecting links any more with allied forms: they are separated from the latter. This character furnishes a good definition for the term 'species,' which ought to be the taxonomic species. This, however, does not mean that in every case it should be easy or even possible to distinguish sharply between a variety and a species, since there are actual cases of transition in nature. Yet at the present state of our knowledge, the insufficiency of the latter alone prevents in many cases a final decision.[10]

[7] See also: Stone, W., in SCIENCE, May 4, 1906, p. 701, with reference to *Viola*.

[8] SCIENCE, June 22, 1906, p. 952.

[9] Darwin, 'Origin of Species,' p. 33.

[10] See *Pr. Amer. Philos. Soc.*, 35, 1896, p. 191.

2. *The essence of de Vries's experiments, pedigree-culture, consists of 'selection' and 'segregation.'* This becomes most evident in MacDougal's description of his methods as given in the paper under discussion (p. 214 f.), and anybody may see at a glance that MacDougal, as well as de Vries, did nothing that has not the purpose of selecting certain variations and their seeds, and of segregating (separating) them from disturbing influences.

Granting that these fundamental views are correct, we may now look at the bare facts represented in de Vries's experiments, without any attempt at explanation or theoretical speculation. The following two stand out prominently:

1. *By pedigree-culture de Vries succeeded in making certain variations breed true.*

2. *In other cases of variations he did not succeed.*

The first sentence tells an old story. The same has been done since times immemorial, and scientific investigation has taken notice of this fact since the time of Darwin. The process is now rather well understood, that is to say, with reference to the essential features of the action required of man: they are selection and segregation. De Vries did not change the old method in the slightest degree, he only introduced additional precaution and refinement in detail, taking particular pains to insure the full efficiency of these two factors by carefully excluding all possible interference with them. In addition, he was the first to keep proper scientific records of what he was doing.

The second fact, on the contrary, is new, and it is the point in de Vries's experiments which needs explanation and a theory. Why is it that certain variations did not breed true under de Vries's hands, although they were treated exactly like those belonging to the first group? The general belief, up to this time, was that any variation might be transformed into a true breeding form by proper treatment.

De Vries's explanation of this fact is given in his mutation theory. Believing that his experiments are conclusive, and that, since he himself did not succeed in cases of the second group, *nobody would be able to do so,* he propounds the theory that there are actually certain variations, in which selection and segregation (pedigree-culture) are impotent to produce true breeding, and, consequently, that there are two classes of variations, the one of which he calls 'mutation,' which produces forms which respond to the effort of the breeder, the other in which the art of the breeder has no effect, and which he calls 'fluctuating variation.' Then the first is, of course, all important for the species-forming process, while the other is of no consequence. There is no saying, with respect to any particular variation, whether it may belong to the one or to the other class, before the actual test (pedigree-culture) has been made, although it seems that mutations often or generally differ from fluctuating variations in the degree of deviation from the original form. This is the essence of the mutation theory.

The above conclusion and theory would be perfectly correct, if the proposition was correct that it is actually impossible to make certain variations breed true. But just in this point, I believe, de Vries is wrong, since his experiments were not conducted in such a way as to absolutely preclude the possibility that even so-called 'fluctuating variations' may be successfully transformed into true breeding forms. We always are to bear in mind that it is at least thinkable that a particular form may be bred true only under particular conditions, under conditions which are congenial or essential to its very existence. To present an imaginary example: a plant species may possess a peculiar variation, which is due to lack of direct sunlight (shade-form). Suppose this shade-form is cultivated according to de Vries's method in the botanical garden, in beds where it gets its full share of sunlight. I never believe, in such a case, that pedigree-culture will succeed in making this shade-form breed true, since always the conditions of environment will have the tendency to paralyze the effort of the breeder. If, however, this particular shade-form is bred in the shade, under proper environment, the attempt possibly may not be in vain.

This is only an example to illustrate what I think may be correct. Every plant breeder

knows that many of our garden forms will come true only when treated in a certain way. These various ways, including everything that comes under the head of gardener's 'tricks,' are familiar to the professional and amateur,[11] and their results are regarded among laymen as due to a 'lucky hand.' Pure strains of seed, of course, obtained by pedigree-culture, are the first condition, but pedigree-culture is not all of the secret, since the proper handling of the seeds is also material, as well as the observation of certain 'tricks' with the growing plants, *which have no other object but to furnish the congenial environment to the object.*

I think that any one who has ever done actual garden work, trying to raise particular strains of flowers or vegetables, will understand what I mean by these 'tricks.' This essential element is obviously lacking in de Vries's experiments: he uniformly bred all his mutants 'in the botanical garden,' and 'in well-manured soil,' and apparently also under the same conditions of climate, season, subsoil, insolation, etc., that is to say, under a uniform set of ecological conditions, such as are generally found in a botanical garden. Indeed, it has been questioned that a change of these conditions may influence the true breeding of a strain, but without sufficient reason, since such an assumption is surely unwarranted as long as the question has not been actually tested in a scientific way. The necessity, in certain cases, to observe certain 'gardener's tricks,' in order to get the best results in raising particular races, strongly favors the opinion that environment actually has something to do with it, and scientific experiments with this in view should be made by all means. Where de Vries succeeded in breeding true his 'mutations,' the environment of the botanical garden was not averse to the experiment, and in this connection it is suggestive that his chief success was attained with *Œnothera lamarckiana*—an escaped garden-form, to which apparently the botanical garden was congenial.[12]

Finally, in order to define my standpoint as precisely as possible, and in order to obviate unnecessary discussion of minor and irrelevant points, I shall condense everything I have said into five questions, and if anybody wants to challenge my propositions, I ask him to do so in terms as laid down here.

1. *Does the 'elementary species' of de Vries correspond to Darwin's conception of 'variety,' and is my definition of 'species' ('taxonomic species') acceptable?*

2. *Are selection and segregation the essential features in pedigree-culture?*

3. *Are de Vries's experiments, aside from their greater accuracy and refinement, essentially identical, in their method, with those of the earlier breeders, as, for instance, recorded by Darwin?*

4. *Is it advisable that breeding experiments should be repeated with due regard to environment, before a final judgment is to be pronounced, and are de Vries's experiments defective on this point?*

If the answer to these four questions is *'Yes,'* then my contentions are recognized as well supported, and the answer to the next question should also be *'Yes'*—

5. *Should the validity of de Vries's mutation theory be doubted, since he makes an unwarranted distinction between two kinds of variation, which further experiments possibly will prove to be identical?*

If, however, anybody should be inclined to answer *'no'* to any or all of these questions, I ask him to give *reasons* for so doing. I have given my reasons for answering them in the

[11] For those who have no practical experience in gardening, the study of a few items in Bailey, 'The Cyclopedia of American Horticulture,' will give an idea of the immense variety of these tricks.

[12] I call attention to Jordan's account of some of Burbank's experiments (in *Pop. Sci. Monthly*, January, 1905), where also the influence of the environment in the production of variation is repeatedly emphasized, chiefly on pages 205 and 206. Possibly, if Burbank's attention is called to it, he may be able at once to quote instances where the true breeding of a certain strain depends largely on the environment offered. This, of course, should be the general rule, but it can be clearly observed only in such cases where a particular feature of the environment is known to be responsible for a particular variation.

affirmative; consequently, it should be demonstrated that my reasons are no good. Nobody ever attempted this, and when arguments were given purporting to be opposed to my ideas, these invariably were not my views but only what the critic fancied to be my views."[13]

A. E. Ortmann
Carnegie Museum, Pittsburg, Pa.,
October 4, 1906

SPECIFIC NAME OF NECTURUS MACULOSUS

In the last number of the *American Naturalist* (Vol. XLI., January, 1907, pp. 23–30) there is an elaborate paper by Professor F. C. Waite under the above title, in which he shows that the name employed there has the priority over *N. maculatus,* the term most commonly adopted by anatomists. Towards the end of the paper (p. 27) he makes the following statement: "In the past ten years although many papers have been written on *Necturus,* two only have, as far as I know, used the correct nomenclature."

I wish to say that the 'correct' name was pointed out and the proper references given by the late Dr. G. Baur as early as 1897 (*Zool. Bull.,* I., p. 41). Since then it has been employed by various systematists. Thus the name *N. maculosus* is used in the eighth edition of D. S. Jordan's 'Manual of the Vertebrate Animals of the Northern United States,' 1899, p. 175, in which I tried to bring the nomenclature up to date. It has since been used, both in this journal (Science, N. S., XI., 1900, p. 555) by Fowler, and in the *American Naturalist* (XL., 1906, p. 159) by Stone.

Leonhard Stejneger
Smithsonian Institution,
January 14, 1907

THE DEFINITION OF SOLID AND FLUID

To the Editor of Science:—The point I have raised (October 26) as to the definition of solid and fluid seems quite timely in view of the discussion going on between Hoskins and See, and the letter of Mr. Willcox (November 9). Note the use of the term 'solid' in one, of 'substance' in the other, of the two definitions of rigidity cited by Hoskins. Their difference seems to be as to whether it is proper to speak of the rigidity of a fluid or a gas. The real question of fact, how much the interior of the earth yields to a certain variation of pressure, has not thus far entered the discussion.

Again, Mr. Willcox defines fluid and solid quite other than was suggested by me and the line between as the curve of the plastic yield point.

His definition is quite tenable, if we agree to it, may be made as exact, and fits quite as well the Latin derivation of the word fluid, but I am not sure that it agrees as well with usage or is as practical. We could then speak of no substance as solid or fluid without knowing under what pressure it is. Whether a body were solid or fluid would then depend not merely on the state of the body itself, including its temperature, but also on its surroundings—the pressure. We cannot, then, as he writes me, 'properly refer to any substance as a plastic solid.'

The earth's interior would be classed as a fluid, and not, as has been lately common, on account of its high rigidity, as solid.

The one point which is not quite clear, as he brings it in parenthetically, is whether the plastic yield point, and so his definition, depends on the time or rate of application of pressure. I judge not, according to the molecular theory which he adopts (dear to T. Sterry Hunt) that there are three states of molecular aggregation, solid, fluid and gas, and that the solid molecules are heavy and complex aggregates of the liquid molecules, as these are in their turn of the gas, and that sufficient temperature and pressure will break up the large solid molecules.

The definition which occurred to me, that a fluid is a body that can not rest under stress, *i. e.,* in a strained condition, is, however, just as definite and draws just as sharp line as that of Mr. Willcox. We may express it in his terms thus—*a fluid has a temperature such that its plastic yield point is reached even at zero pressure.* The relative content of the two concepts can be expressed graphically thus.

[13] See also my reply to Gager's criticism in Science, August 17, 1906, pp. 214–217.

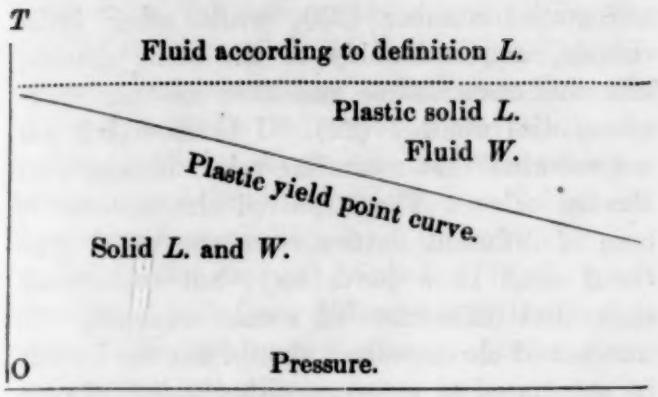

Figure illustrating varying conceptions, as to where the line between fluid and solid should be drawn.

Neither definition is absolutely rigorous, perhaps. It may be that every fluid can rest under a very minute amount of strain, and that every solid has plasticity.

No discussion of facts can settle usage, so I call for others, either here or in the scientific meetings, to express themselves as to what usage will best fit that of the past and present, and be most practical for the future.

ALFRED C. LANE

A CORRECTION

TO THE EDITOR OF SCIENCE: To a communication by the writer, which appeared in SCIENCE for January 18, 1907, the name of the U. S. Geological Survey was attached without authority of the director. The writer wishes to record his disavowal of any desire to commit the survey to an indorsement of the sentiments expressed in said note. For these he alone is responsible. In explanation he would add that the note was written before the writer became a member of the U. S. Geological Survey. It was not offered for publication, however, until about the time he was planning to enter on field work, when his new address was attached without due consideration.

Moreover, on deliberate reading, the writer is conscious that unintentionally there appears to be in the language employed a tone of discourtesy, which he regrets.

C. H. GORDON

January 22, 1907

SPECIAL ARTICLES

THE CASE OF ANASA TRISTIS

IN a paper read before the December meeting of the American Society of Zoologists in New York, illustrated by a very beautiful series of photomicrographs taken from smear-preparations, Miss Foot and Miss Strobell announced the following conclusions, which have since been published in a preliminary form in the January number of the *Biological Bulletin:* (1) There is no odd or 'accessory' chromosome in *Anasa tristis.* (2) The number of spermatogonial chromosomes is 22. (3) All the chromosomes divide in both maturation divisions. The so-called odd or accessory chromosome is only a 'lagging' chromosome, and it divides with the others in the second division. (4) The so-called 'chromosome-nucleolus' of the growth period is not a chromosome, but a nucleolus.

These results are at variance with my own, and since the differences in regard to the first three involve the important more general issue of the relation of the chromosomes to sex-production, I will make the following reply.

The fourth of the above conclusions, though materially different from my own, is not altogether irreconcilable with it. I have for some time had reason to suspect (in case of certain other genera) that a stage may have been overlooked in the prophases in which the odd chromosome temporarily loses its compact nucleolus-like form. For the study of this question smear-preparations offer decided advantages; and I am ready enough to admit that in regard to these stages Miss Foot and Miss Strobell may have made an important addition to our knowledge, though I still believe that the chromosome-nucleolus of the earlier stages is the odd chromosome. On the other and more vital points their results are irreconcilable with my own, and only these will further be considered here.

Since the announcement of these results I have carefully reexamined my old preparations (including those of Paulmier) and a series of new ones from material collected during the past summer. They include sections of material fixed in Flemming's, Her-

mann's, Bouin's and Gilson's fluids, of both *Anasa tristis* and *A. armigera,* that of the first-named species from Massachusetts, New Jersey, Michigan and Arizona. The reexamination not only confirms but fortifies anew at every point my former results. The evidence given by these preparations is, in my judgment, completely demonstrative that in *Anasa tristis:* (1) There is an odd chromosome that is one of the largest three spermatogonial chromosomes. (2) The number of chromosomes in the spermatogonia is 21, in the ovarian cells 22. (3) The odd or 'accessory' chromosome divides in the first spermatocyte division, but passes undivided to one pole in the second. Half the spermatozoa thus receive 10 chromosomes and half 11.

These facts appear with irreproachable clearness in a great number of cells, and with a uniformity of result that I think precludes the possibility of error. The number, size and grouping of the spermatogonial chromosomes, and the history of the odd chromosome in the second division, are clearly shown in Dr. Leaming's photographs (which will be published hereafter, should it seem desirable); and both these and the preparations are at the service of any who may wish to examine the evidence on which my conclusions rest. I may add that an odd chromosome also exists in the following genera of coreids that have not hitherto been reported on (the spermatogonial numbers in brackets): *Pachylis* (15), *Euthoctha* (21), *Narnia* (21), *Leptoglossus* (21), *Chelinidea* (21), *Margus* (23), *Catorintha* (25), *Leptocoris* (13), and in the pyrochorid genus *Largus* (*L. succinctus,* 13, *L. cinctus,* 11). In *Catorintha,* as in *Protenor,* the odd chromosome is the largest of the chromosomes. In *Metapodius,* alone among the coreids thus far examined, a pair of unequal idiochromosomes appear in place of the odd chromosome.

What is the explanation of this contradiction of results? Is *Anasa tristis* a kind of cytological Jekyll and Hyde which presents itself in different guise to different observers? Possibly; for I have found in *Metapodius terminalis* that certain individuals possess a small unpaired chromosome and an odd spermatogonial number (23), while other individuals, unquestionably of the same species, lack this chromosome and have an even spermatogonial number (22). I believe (but am not certain) that a similar relation occurs in *Banasa calva.* The unpaired chromosome is here of different nature from the usual type (as I shall show hereafter); but such cases show that differences of result regarding the number of chromosomes should not too hastily be attributed to error on either side. As regards *Anasa,* I can only state that the same result is uniformly given by many different individuals, from widely different localities (including Woods Hole, where the material of Miss Foot and Miss Strobell was procured) and after all the methods of fixation. The contradiction is, I think, probably due to the difference of method employed, Miss Foot and Miss Strobell having placed their faith in smear-preparations, while I have relied on sections. Without undertaking any discussion of the relative merits of the two methods, I may say that whatever be the disadvantages of sections for the purposes of photomicrography, really well fixed and stained sections are certainly competent for the demonstration of such points as are here at issue, while they have the advantage of leaving the topographical relations undisturbed. But the certain determination of the number of chromosomes in a given species by means either of smears or of sections, demands a more critical treatment than the mere matter of counting (or photographing) the number that may lie in a given field of view or even in many fields. The real task is to determine the relation normal to the species by the elimination of occasional variations (such as certainly occur in some species) and of plus or minus errors due to accident; and this again is not merely a matter of frequency of occurrence, but also of evidence given by the nature of the chromosome-groups taken in detail and as a whole. As far as sections are concerned, it is almost too elementary to require mention that a minus error may arise from the failure of one or more of the chromosomes to lie within the plane of section. This might be due either to a position that is normally at a different

level from the others, or to accident. The first of these possibilities is eliminated by side views, which show that at full metaphase the chromosomes lie in a flat plate; while the early anaphases show the daughter chromosomes separating in two flat parallel plates. The second possibility is, I think, wholly excluded in the case of *Anasa* by (1) the great number of cells from different individuals that show 21 chromosomes (often a smaller number, in my experience never in a single instance 22); (2) the equally constant appearance of 22 chromosomes in the female; and above all (3) the fact that in *Anasa* and some of the other genera *the smaller number in the male is shown by the size relations to be consistently owing to the absence of a particular chromosome*—in *Protenor* always one member of the largest pair that appears in the female, in *Anasa* always a member of one of the largest two pairs. It is manifestly impossible that this should be due to accident. As to the passage of the lagging odd chromosome without division to one pole in the second division, I do not think the most critical observer who examines the demonstrative evidence given by very large numbers of cells in my preparations can have the slightest doubt.

I, therefore, think that as far as the first three points are concerned, my results on *Anasa* will sooner or later receive full confirmation at the hands of other observers. Paulmier himself, after reexamining his own preparations, was thoroughly convinced of his error regarding the spermatogonial number—an error that was a natural one at the time his work was done. The most careful search has failed to discover the original of the group from which he figured 22 chromosomes, or any other agreeing with it in number. Montgomery's confirmation of my result as opposed to his own earlier one is known. The consistent and cumulative corroboration given by so many other genera of Hemiptera (including one of the Homoptera recently studied by Miss Stevens), some of them far more favorable for study than *Anasa*, speaks for itself.

EDMUND B. WILSON

ZOOLOGICAL LABORATORY,
COLUMBIA UNIVERSITY,
January 20, 1907

NOTES ON ORGANIC CHEMISTRY

DIAZONIUM PERCHLORATES

THE chemical activity of perchloric acid and the fact that, under certain conditions, it is explosive, are matters of common knowledge, it is also generally known that many of the diazonium (diazo) salts, such as benzenediazonium nitrate, $C_6H_5N:NNO_3$, are likewise explosive and their instability is often very great. It might be anticipated, therefore, that diazonium perchlorates, if they could exist at all, would prove to possess an unusual degree of energy and that their study, although it might be attended with danger, would yield results of considerable general interest. A number of such compounds have recently been described, simultaneously, by D. Vorländer,[1] and by K. A. Hofmann and H. Arnold.[2] The object of the former was to endeavor to discover some basic substance which, with perchloric acid, would form a sparingly soluble salt that could be employed instead of potassium perchlorate for the quantitative determination of the acid. This result has not been attained, but it is found that an immediate, voluminous precipitate is formed when phenylacridine sulphate is added to a 1 per cent. perchloric acid solution, and that a turbidity is produced when a 0.1 per cent. solution of the acid is used. Hofmann and Arnold desired to obtain some sparingly soluble diazonium salts. The preparation of *benzenediazonium perchlorate*, $C_6H_5N:NClO_4$, is very simple. Aniline (2 grams) is mixed, at a temperature of 0°, with water (200 c.c.), concentrated hydrochloric acid (6 c.c.), commercial perchloric acid (10 c.c.) and sodium nitrate (1.5 grams). The perchlorate immediately crystallizes out in colorless needles, which are strongly doubly refractive. Even in the moist state this compound explodes with great violence if rubbed or struck with articles of stone or iron. When dry the explosive power of the substance is, of course, very much greater. If a few centigrams are dropped on blocks of hard wood, deep holes are torn in them, but the explosion is so local in its effect that vessels of thin glass, placed a

[1] *Ber.* **39**, 2713 (1906).
[2] *Ibid.*, **39**, 3146 (1906).

few inches away from the seat of disturbance, are uninjured.

The sensitiveness of the orthotoluene derivative is even greater than that of the above benzene compound, because in the moist state it explodes if it be touched with a porcelain stirring rod.

Chemically, these compounds are relatively stable and can be retained in a cool place for a day without change.

A considerable number of other diazonium perchlorates are described in the two papers quoted; in general, they exhibit the interesting contrast of being slightly *less* explosive than the two salts described above, but considerably *more* unstable chemically.

PREPARATION AND PROPERTIES OF BENZOYL NITRATE

The formation of an acid chloride, such as acetyl chloride, CH_3COCl, from an acid and phosphorus pentachloride, is one of the most generally applicable reactions known. The corresponding bromides are available in moderate numbers, but only a very few of the iodides or fluorides have been prepared and, with these exceptions, no compounds are known containing an inorganic acid radicle in place of chlorine. For this reason, as well as on account of its properties which are described below, the isolation of *benzoyl nitrate*, $C_6H_5COONO_2$, is a matter of considerable interest. A preliminary announcement on the subject some months ago by F. Francis[1] has been followed up with a fuller account by him self[2] and T. H. Butler.[3] The compound is prepared by mixing together, at about $-15°$, benzoyl chloride and well-dried, finely divided silver nitrate. The product is an oil which is extremely sensitive to moisture. It may be filtered through dry filter paper, but if the latter contains the ordinary amount of moisture the nitrate reacts with explosive violence. In sealed tubes, at the ordinary temperature, the nitrate changes gradually into benzoic anhydride and, apparently, nitrogen pentoxide. At 100° the change is similar, except that nitrogen peroxide and oxygen are formed, and at higher temperatures the reaction is so rapid as to produce an explosion. In nitrobenzene solution the nitrate is transformed slowly, to the extent of 60 per cent. in six months, into metanitrobenzoic acid. Other non-ionizing solvents bring about the same change, but less rapidly.

In its reactions benzoyl nitrate behaves as a nitrating agent, as an oxidizing agent, or as a means of introducing a benzoyl radicle. Alcohol yields ethyl nitrate; aromatic hydrocarbons, phenols, and their ethers give, with ease, chiefly mono-nitroderivatives. As examples of oxidation may be mentioned the transformation of thiophenol and hydrazobenzene into diphenyldisulphide and azobenzene, respectively. Primary aromatic and secondary aliphatic amines yield benzoyl derivatives, C_6H_5CONHR, but secondary aromatic amines form nitrosamines. With the exception of paratolylmethylnitramine, $CH_3C_6H_4N(NO_2)CH_3$, these are unstable and pass into nitro secondary amines, such as $CH_3C_6H_3(NO_2)NHCH_3$.

One of the chief points of interest in connection with the nitrating action of benzoyl nitrate is that it permits of this reaction being realized in the complete absence of water, a condition which, hitherto, has been unattainable. The nearest previous approximation to it was by the use of diacetyl orthonitric acid, $(CH_3COO)_2N(OH)_3$. One result of such change in the conditions of reaction is that orthonitroaniline, which can only be obtained to the extent of about 10 per cent. in admixture with its isomers by the ordinary methods of nitration, is formed quantitatively by the action of benzoyl nitrate on acetanilide.

Metanitrobenzoyl nitrate, $O_2NC_6H_4COONO_2$, and *butyryl nitrate*, $CH_3CH_2CH_2COONO_2$, are also formed by the action of the respective chlorides on silver nitrate, hence it would appear that such compounds are always produced, as intermediate steps, in the reaction of an acid chloride on a metallic nitrate.

J. Bishop Tingle
Johns Hopkins University

[1] *Jour. Chem. Soc.*, **89**, 1 (1906).
[2] *Ber.*, **39**, 3795 (1906).
[3] *Ibid.*, **39**, 3804 (1906).

QUOTATIONS

THE BIOLOGICAL SURVEY

WHAT Representative Wadsworth of New York, chairman of the House Committee on Agriculture, hopes to do by striking out from the agricultural bill before it is reported to the House the provisions for the maintenance of the biological survey is not clear. If this menace is agreed to by the House it will only serve to add to Mr. Wadsworth's unpopularity with the farmers of his section of the state. On the face of it, the plan to abolish the biological survey is an utterly foolish thing to do.

Because no reason is apparent for the hostile action of Mr. Wadsworth's committee, some persons have suspected a personal animus back of the move. If the appropriation for the support of the survey is allowed to be stricken out on the grounds of economy it is a policy of penny wise and pound foolish. It costs only about $55,000 to do the work of the survey. Naturalists, bird lovers, and all others interested in game protection say that the work of the bureau is done efficiently.

Dr. C. Hart Merriam, the chief of the biological survey, is a New York physician, who gave over a large practice in order to take up the government work at a small salary because of his love for natural science. He is one of the noted ornithologists and mammalogists of the world. Dr. A. K. Fisher, assistant in charge of economic investigation, is also a physician who dropped his practice and went in to the government service for reasons akin to those which moved Dr. Merriam to the same course. Dr. T. S. Palmer has direct charge of the enforcement of the game laws and is the chief adviser on game and song-bird protection matters for the game protection officials of the different states. Under Dr. Palmer's direction within the last few years the work of protecting the game birds and game mammals of the country has been put upon a plane of efficiency.

The present expectation is that the usual appropriations for carrying on the work of the survey will be put in while the bill is under consideration in the House. Even should representatives support their committee, not much doubt is expressed that the senate will do what is necessary. President Roosevelt is not loath to take a hand in legislative enterprises, and he would certainly not be backward in coming to the assistance of the chief of the survey should his assistance be necessary.—New York *Evening Post.*

LECTURES ON PROBLEMS OF INSANITY

THE Psychiatrical Society of New York has arranged for a series of four lectures on problems of insanity, to be held under the auspices of the Academy of Medicine at 17 West 43d Street, on Saturdays, January 19, February 2, February 16 and March 2, 1907, at 8:30 P.M. The purpose of these lectures is to put within the reach of the medical profession and also of the non-professional leaders of sociological interests a program of work and facts for orientation, with a view to the organization of a movement towards prophylaxis and the development of sound interests in this eminently important topic.

The first lecture will be given by Dr. Adolf Meyer, on Modern Psychiatry, its possibilities and opportunities; the second lecture, by Dr. August Hoch, discusses the manageable causes of insanity, exclusive of heredity; the third lecture by Dr. C. L. Dana, the data of heredity and their application in psychiatry; and the fourth lecture by Dr. Allan McLane Hamilton, the development of the legal regulations concerning the insane.

Encouraged by the welcome which the broad movement against tuberculosis has received, the society considers a public discussion of the facts of insanity of fundamental importance for a natural development of public and personal hygiene, and the only way to replace the traditional horror and disregard by a profitable interest on the part of the thinking and active citizens of the community. Where general cooperation is so much needed as it is in the handling of abnormal mental developments, it is especially necessary to bring together the many interests which now work independently in social reform, schools, hospitals, courts and institutions. C. MACFIE CAMPBELL, M. B.,

Secretary

THE ERECTION OF A MONUMENT TO THEODOR SCHWANN

On the seventh of December, 1910, a century will have elapsed since the day that Theodor Schwann was born at Neuss on the Rhine.

His native town has resolved to raise to this great son of hers a monument to be unveiled on that memorable day. An appeal to the public having been made to this effect by the physicians of the Rhine country and the medical faculty of the University of Bonn, a considerable sum of money has been contributed both by the inhabitants of Neuss and especially by the physicians of Rhineland and Westphalia.

A primary fund for the realization of the project being thus secured by his countrymen, we may be pardoned if we address to the biologists and especially to the physicians all the world over the urgent request, to enable us by their generosity to accomplish a work destined to glorify one of theirs, and of whom they may feel justly proud.

Theodor Schwann's merit, we need scarcely say, is so eminent, that it has been equalled by few savants, surpassed by none. It was he who, sixty-five years ago, joined his efforts to those of M. J. Schleiden, and under the auspices of his master, Johannes Müller, he was the first to substantiate the cellular theory. In the course of time he proved to be a pioneer no less successful than indefatigable in the domains of histology, physiology and biology; an authority on the processes of fermenation, decomposition, spontaneous generation and digestion and last, not least, the discoverer of pepsin.

Every gentleman and friend of scientific progress satisfies, therefore, but a duty of honor in contributing for a monument in honor of Schwann.

Whosoever had the good fortune of knowing this modest man in his life and of looking in his gentle and intelligent eye, will know that his heart was not set on exterior things. Ever ready to acknowledge the merit of others, he aspired to no honors for himself: all the more reason to honor him in his death.

To his immortal master, Johannes Müller, a statue of bronze has been lately erected in his native town of Coblenz; his collaborator Schleiden has been similarly honored at Jena, the chief scene of his labors. Let us, therefore, not be remiss in discharging our debt of gratitude to Schwann whose name is linked preeminently and forever to the substantiation of the cellular theory, one of the greatest achievements of modern science.

Contributions are, please, to be addressed to the 'Städtische Sparkasse zu Neuss am Rhein. Schwanndenkmal, Germany.'

Germany.—Berlin: Geh. Rat Professor Dr. Waldeyer, Geh. Rat Professor Dr. Hartwig, Geh. Rat Professor Dr. Franz Eilhard-Schulze, Professor Dr. Posner. Bonn: Geh. Rat Professor Dr. Pflüger, Geh. Rat Professor Dr. von la Valette St. George. Breslau: Geh. Rat Professor Dr. K. Hasse, Professor Dr. K. Hürthle. Erlangen: Professor Dr. Hauser, Professor Dr. Fleischmann. Freiburg i. Br.: Geh. Rat Professor Dr. Bäumler. Giessen: Professor Dr. K. Eckhard, Professor Dr. H. Strahl. Göttingen: Geh. Rat Professor Dr. Ebstein, Geh. Rat Professor Dr. Esser. Greifswald: Professor Dr. Bleibtreu, Geh. Rat Professor Dr. Mosler. Halle: Professor Dr. Roux, Professor Dr. Grenacher. Heidelberg: Geh. Rat Professor Dr. Fürbringer, Professor Dr. A. Kossel. Jena: Hofrat Professor Dr. von Bardeleben, Professor Dr. Häckel. Kiel: Professor Dr. Graf F. Spee, Professor Dr. Hensen. Königsberg: Professor Dr. Stieda, Professor Dr. L. Hermann. Leipzig: Professor Dr. Babl, Professor Dr. Hering. Marburg: Professor Dr. Gasser, Professor Dr. F. Schenk. München: Professor Dr. Rückert. Münster, Professor Dr. Ballowitz. Rostock: Professor Dr. Barfurth, Professor Dr. Langendorff. Strassburg: Professor Dr. Schwalbe, Professor Dr. Hofmeister. Tübingen: Professor Dr. Froriep, Professor Dr. Grützner. Würzburg: Professor Dr. Rindfleisch, Professor Dr. Th. Stöhr.

Austria.—Graz: Professor Dr. Holl, Professor Dr. Zoth. Innsbruck: Professor Dr. Hochstetter. Prag: Professor Dr. Siegmund Mayer, Professor Dr. J. Gsad. Wien: Hofrat Professor Dr. Ritter von Ebner, Hofrat Professor Dr. E. Zuckerkandl.

Switzerland.—Basel: Professor Dr. Kollmann, Professor Dr. Metzner. Bern: Professor Dr. Strasser, Professor Dr. Kronecker. Genf: Professor Dr. Laskowski, Professor Dr. Prevost. Lausanne: Professor Dr. Bugnion, Professor Dr. Herzen. Zürich: Professor Dr. Ruge, Professor Dr. Gaule.

Belgium: Professor Dr. Swaen, Professor Dr. Ed. van Beneden, Professor Dr. Frédéricq.

Denmark: Professor Dr. F. C. C. Hansen.

England: Professor Dr. Edw. Albert Schaefer.

Greece: Professor Dr. Sclavúnos.

Holland: Professor Dr. Pekelharing.

Italy: Professor Dr. Guglielmo Romiti.

Portugal: Professor Philomano da Camara.

Russia: Professor Exz. Tarenetzki.

Sweden: Professor Dr. G. Retzius.

Norway: Professor Dr. Guldberg.

Spain: Professor Dr. S. Ramon Cajal.

Hungary: Professor Dr. von Lenhossek.

United States: Professor Dr. Charles S. Minot.

Canada: Professor Dr. Ramsay Wright.

Chile: Professor Dr. Jzquierdo.

Argentine: Professor Dr. Rodolf de Gainza.

Brazil: Professor Dr. Jhering.

Mexico: Professor Dr. Fernando Altamivano.

Asia—Calcutta: Professor Captain J. B. Kelly.

Japan: Professor Dr. J. Kaganii.

Australia: Professor Dr. James F. Wilson.

SCIENTIFIC NOTES AND NEWS

DR. CHARLES D. WALCOTT, director of the U. S. Geological Survey, was elected secretary of the Smithsonian Institution by the regents at their meeting on January 23.

M. CHAUVEAU, of the section of agriculture, has been elected president of the Paris Academy of Sciences to succeed M. Poincaré, of the section of mathematics.

M. BOUQUET, director of technical instruction in the French Ministry of Commerce, has been appointed director of the Conservatoire National des Arts et Métiers, to succeed M. Chandèze, who has retired from active service.

PROFESSOR A. MÖLLER has been appointed director of the Forest Academy at Eberswald.

PROFESSOR W. G. FARLOW, having withdrawn from the American editorship of the *Annals of Botany,* Professor Roland Thaxter has been chosen as his successor.

MR. ROOSEVELT has been elected one of the ten honorary members of the Royal Geographical Society.

DR. THOMAS A. STOREY, director of the gymnasium of the College of the City of New York, has been elected to the presidency of the Society of College Gymnasium Directors of the United States.

PROFESSOR CHARLES W. BROWN, of Brown University, has gone to Jamaica to study the geological phenomena connected with the recent earthquake.

MR. ARCHER M. HUNTINGTON has been elected president of the American Geographical Society in succession to Commander Robert E. Peary.

DR. GEORG SCHWEINWURTH, the African explorer and botanist, celebrated on December 39 his seventieth birthday.

PROFESSOR JULIUS ARNOLD, who has held the chair of pathological anatomy at the University of Heidelberg, has retired from active service.

DR. THEODOR SCHOTT, of Nauheim, will give lectures in Boston and elsewhere on diseases of the heart.

PROFESSOR ANGELO HEILPRIN lectured before the Franklin Institute on January 17, on 'Conceptions regarding Earthquake Phenomena and the Relationship of these Phenomena to Volcanic Disturbances.'

DR. S. A. MITCHELL, of Columbia University, will lecture at Vassar College on March 1, on 'Personal Experiences in Spain at the Recent Total Eclipse.'

PROFESSOR FRANK H. BIGELOW, of the U. S. Weather Bureau, gave, on January 22 and 23, two public lectures in meteorology at the University of Chicago, under the auspices of the departments of geography and economics. His topics were 'The Circulation of the Sun's Atmosphere as the First Cause of the Annual

Changes in the Weather,' and 'The Circulation of the Earth's Atmosphere, and the New Theory of Storm Energy.'

THE winter course of free and illustrated popular lectures for 1907 of the Chicago Academy of Sciences will be given in the Assembly Hall of the Matthew Laflin Memorial Building, Lincoln Park, on Friday evenings, at eight o'clock, as follows:

January 25, 'American Museums and their Work,' Frank Collins Baker, Curator of the Academy.

February 1, 'The Evolution Theory, Pro and Con,' Dr. Winfield Scott Hall, Professor of Physiology, Northwestern University Medical School.

February 8, 'Deep Sea Fishes and Fishing,' Dr. Seth E. Meek, Assistant Curator, Department of Zoology, Field Museum of Natural History.

February 15, 'The Every-day Application of Ohm's Law,' a discussion of elementary electricity, Dr. Howard N. Lyon, Chicago.

February 22, 'The Place of Bacteria in the Causation of Disease,' Dr. Adolph Gehrmann, Bacteriologist, Columbus Medical Laboratories, Chicago.

March 1, 'Other Worlds than Ours,' Dr. Forest Ray Moulton, Assistant Professor of Astronomy, University of Chicago.

March 8, 'The Work of Leaves,' Dr. Charles B. Atwell, Professor of Botany, Northwestern University.

March 15, 'From the Big Horn Basin to the Yellowstone and Jackson's Hole *via* Sylvan Pass,' Mr. Charles A. Heath, Chicago.

MR. WILLIAM WELLS NEWELL, of Cambridge, Mass., known for his researches in folk-lore, especially in connection with the Authurian tales, secretary of the American Folk-lore society and fellow of the American Association for the Advancement of Science, died on January 21, at the age of sixty-eight years.

PROFESSOR DAVID IRONS, professor of philosophy at Bryn Mawr College, died suddenly on January 24. He was born in Scotland in 1870, and received the M.A. at St. Andrews in 1901 and the doctorate of philosophy from Cornell University, 1894. After holding various positions in Cornell University, he was elected professor of philosophy in Bryn Mawr College in 1900. He was the author of contributions to ethics and psychology.

CHARLES B. SIMPSON, entomologist of the Department of Agriculture of the Transvaal, died of typhoid at Pretoria on January 14. Mr. Simpson was a graduate of the University of Idaho, and took his master's degree at Cornell University. In 1900 he became connected with the Bureau of Entomology of the U. S. Department of Agriculture as a special field agent, and was placed in charge of the investigation of the codling moth in the northwest. His investigations were brilliantly successful and resulted in the publication of Bulletins Nos. 35 and 41 of the bureau. In 1904 he was selected to fill the post of entomologist of the Department of Agriculture of the newly created Transvaal colony, and has since resided in Africa, making important investigations on malaria, on locust damage, and on the tick-borne diseases of cattle.

DR. CLEMENS AUGUST SCHLÜTER, for the past twenty-five years professor of geology and paleontology at Bonn, died on December 25, at the age of seventy-one years.

PROFESSOR OTTO BENNDORF, director of the Archeological Institute of the University of Vienna, has died at the age of sixty-eight years.

A BILL has been introduced in the legislature incorporating the New York Observatory and Nautical Museum, to which reference has already been made in SCIENCE. It is stated in the charter that the museum is "for the purpose of encouraging and developing the maritime interests of New York City, of advancing the general knowledge of the safe navigation of the sea, of the development of harbor facilities, of prosecuting original researches in astronomy and navigation and in kindred subjects, and of affording instruction in the same." The incorporators are Frederick G. Bourne, Cornelius Vanderbilt, John E. Bourne, Edward S. Isham, J. D. Jerrold Kelley, Allison V. Armour, Edward H. Wales, George A. Cormack, John Neilson, Charles Lane Poor, William M. K. Olcott, Edward D. Adams, D. Delahanty and Addison Brown. Substantially the museum would be placed on the same basis as the Museum of Natural

History and the Metropolitan Museum of Art. The city is to provide the land and is to erect the buildings, while the corporation is to secure by private subscription not less than $300,000 for equipping the nautical museum and observatory and for prosecuting the other objects of the institution.

THE different chapters in future volumes of the series 'Mineral Resources of the United States,' which the U. S. Geological Survey has been publishing for the past twenty-one years, will hereafter be written by members of the survey. In this report they will differ from their predecessors, which have been prepared mainly by outside experts for each of the mineral industries, working under appointments as special agents of the survey. This corps of experts included Messrs. Charles Kirchhoff, George F. Kunz, James M. Swank, Joseph D. Weeks, Charles D. Yale, John Birkinbine, Joseph Hyde Pratt, F. H. Oliphant, Heinrich Ries, Edmund Otis Hovey and F. J. H. Merrill. In the introduction to the last volume, which covers the calendar year 1905, Dr. David T. Day, chief of the division of Mineral Resources, in making this change, acknowledges his great indebtedness to them for their monumental work in building up and maintaining this series and his regret at the breaking of the ties of a score of years of joint endeavor. This series of reports was begun in 1883. Its object then, as now, was to give an account of the known mineral resources of the United States and present a statistical statement of the production of these materials and the uses to which they were applied. At that time the study of economic geology in the United States Geological Survey was greatly limited by insufficient appropriations from congress and by the fact that the training possible for an economic geologist in the leading colleges and in the geological survey itself was not sufficient to secure an efficient corps of trained men for studying in any comprehensive way the mineral deposits of the entire country. The cooperation was therefore invoked of those who were recognized as the best experts of the country for each of the mineral industries. In most cases this work was entered upon by the experts without the possibility of obtaining any such compensation as they would have demanded for private reports. In general, their services have been practically for an honorarium sufficient only to cover the clerical aid.

THE London *Times* states that considerable additions have been made during the past year to the natural history collections of the Bristol Museum. The skin of Rajah, the fine tiger which lived for about ten years in the Clifton Gardens, was presented to the museum, for which it was well mounted by Rowland Ward, who also supplied about a dozen nesting groups of British birds, arranged on the plan in vogue at the Natural History Museum. The local collection of birds has been increased; Mr. H. J. Charbonnier presented a fine collection of two-winged flies, and another of bees and ants, collected in the district; and the display of game trophies has been made more interesting by collections of heads, horns and antlers deposited on long loan by friends of the museum.

WE learn from *Nature* that the preliminary program of the second International Congress on School Hygiene, to be held on August 5–10, 1907, at the University of London, South Kensington, has been issued. The work of the congress will be divided into eleven sections, each presided over by an authority on the subject dealt with. The organizing committee is inviting educational and public health authorities, universities, colleges, schools, societies and others to appoint delegates to the meeting, and is appealing for donations to meet the large expenditure involved in organizing the congress, which it is estimated will be not less than 3,000*l*. The president of the congress is Sir Lauder Brunton, F.R.S., and the hon. secretaries are Dr. James Kerr and Mr. E. White Wallis.

NOTING that all persons in the United Kingdom whose incomes exceed $850 per annum are assessed for income tax, Consul R. W. Austin, of Glasgow, summarizes an official report just issued, showing the amounts assessed for the year ended April 5, 1905, which indicates the various gross incomes in that

country. Under the heading of businesses, professions, and employments, it appears that there were assessed 456,571 persons with gross incomes amounting to $624,349,605; 57,244 firms with a gross income of $420,244,765; 30,129 public companies with a gross income of $1,204,046,240, and 9,582 corporations and other local authorities (assessed for profit or interest) with a gross income of $99,068,330. The total gross income was $2,347,708,940. Dealing with the incomes of individuals the report shows that there were assessed in Great Britain 6,137 persons with incomes over $5,000 and not exceeding $10,000; 1,405 with incomes of over $10,000 but below $15,000; 533 with incomes over $15,000 and below $20,000; 304 over $20,000; 442 with incomes over $25,000 but not exceeding $50,000; 212 with over $50,000 but not exceeding $250,000; and 24 whose incomes exceeded $250,000.

According to a foreign journal, Dr. Erich Zugmayer, a young naturalist who has already gained some experience as a traveler during a journey in Western Asia, described in his book, 'Eine Reise durch Vorderasien,' has lately undertaken an expedition to Tibet, which he hopes to cross from north to south, returning through India. He had reached Polu, on the southern border of Chinese Turkestan, in June, 1906, having traveled by the newly opened trans-Aral railway to Tashkend, and thence proceeded *via* Kashgar, Yarkand, and Khotan. From the last-named place he had executed a route survey, making also astronomical and meteorological observations in addition to those relating to his more special subject, natural history. He had already sent back from Polu three cases full of the collections made during the earlier part of the journey. From Polu he hoped to cross the Shu-bashi pass, and to examine an area hitherto blank on the map before striking the route of Dutreuil de Rhins.

UNIVERSITY AND EDUCATIONAL NEWS

Through the recent death of the last child of the late John C. Frye, a Boston produce merchant, Tufts College comes into a bequest of $200,000.

It is announced that the late Dr. Gustave Schorstein bequeathed 500*l.* to the regius professor of medicine at the University of Oxford for the pathological department of the medical school to the London hospital, and a sum, which will amount to some 10,000*l.*, in trust to the University of Oxford, subject to certain life interests.

Nature says: "The report of the Board of Education for the year 1905–6 is of an encouraging nature. There is plenty of evidence provided that our national system of technical education continues steadily to improve. The report points out that much attention has been paid throughout the country to the extension and improvement of the facilities provided for continuative education. There has been marked activity in the establishment of courses of instruction affording special technical training, and the effective character of the many courses organized under varied conditions shows that local circumstances have received the consideration necessary for success in this kind of educational work. Technical institutions affording whole-time training for those who can give two or more years to study after completing a secondary school course have improved and multiplied their courses of technical instruction. The multiplication of courses, requiring the whole time of students is a gratifying indication of the growing appreciation of the value of the work of the technical school; but this appreciation is not confined to whole-time instruction. The improved organization of the varied institutions engaged in supplementing the training which a youth receives in the office or workshop has borne fruit in many practical developments, demonstrating the extent to which such further education may become a recognized element in the lives of our youths. The report, which runs to 106 pages, deals fully with every department of elementary, secondary and technical education, and shows conclusively that, political controversy notwithstanding, valuable work is being accomplished in the schools."